THE

QUARTERLY JOURNAL.

December, 1826.

Art. I.—*On the Native Black Oxide of Manganese, with some Observations on the Peroxide of Lead, and on Chlorine. By* Mr. J. Mac Mullen.

[Communicated by the Author.]

Of the various substances which modern chemistry has rendered subservient to the arts, there are few more interesting to science, or more important to the manufacturer, than the native black oxide of manganese. Of this valuable mineral extensive mines exist in several parts of the British Empire, the most important of which are found in Devonshire; others have been recently worked in Warwickshire and Derbyshire, and one, the extent of which has not been at all ascertained, is met with a few miles from Dublin, on the south-western shore of the promontory of Howth.

The value of the different specimens of manganese obtained in these widely-separated districts (as far as manufacturers are enabled to ascertain it) is various; and the price of the pulverized oxide of commerce consequently fluctuates in a corresponding ratio, some descriptions being sold ten, twenty, or even thirty per cent. beyond the price of others.

My practical acquaintance with the subject has chiefly arisen from the circumstance, that I have been for several years extensively engaged in the manufacture of the well-known chloride of lime, or bleaching-powder; and my researches into the nature and properties of manganese were principally prompted by the necessity, of which I was early convinced, that in order to secure success in this manufacture, it was indispensable that I should be enabled to form a tolerably correct estimate of the quality of the oxide which I employed; or, in other words, that I should be enabled to determine, with a reasonable degree of accuracy, the proportion of oxygen which was combined with it. To accomplish this object I had recourse to various expedients, the detail of which would be as tedious as unnecessary. One simple *formula*, however, experience taught me might, to a considerable extent, be relied on, and I have since uniformly practised it. It consists in merely placing a pound of the pulverized oxide in a tall glass vessel, pouring upon it four times its weight of dilute sulphuric acid, of known strength, and allowing the mixture to remain in this state for some days, agitating or stirring it for a few minutes at regular intervals. In comparative trials by this test, I have uniformly found that the particular oxide which imparted to the sulphuric acid the *deepest crimson colour*, was that which in practice was the most productive, and consequently the most profitable. It will be readily supposed that I resorted to this test frequently and attentively; and it is to the results which accident and observation taught me to deduce from it, that I owe the facts and the reasoning which I now take occasion to detail.

For these comparative trials I used cylindrical flint-glass cans, eight or nine inches in height, and about three inches in diameter: the manganese having been put into one of these vessels, and the dilute sulphuric acid poured over it, I was enabled, in the course of a few days, to observe the degree of colour which was imparted to the liquor, and, consequently (according to the test I had adopted), to judge of the quality of the manganese employed.

The smell which is exhaled by the mixture of black oxide of manganese with sulphuric acid, has been long noticed for its peculiarity; and I did not fail to remark it.

On several of these occasions the mixture of manganese and sulphuric acid, which had been employed, was allowed to remain undisturbed for a considerable time after the object, for which it had been prepared, was attained. After the lapse of some months I happened to pour from one of these mixtures the supernatant acid, and, to ascertain if any change in the smell of the vapour which escaped from it had occurred, I agitated the residuum. To my extreme surprise, a pungent and overpowering vapour of chlorine was immediately evolved, the odour of which, perfectly familiar to me, I could not for a moment mistake. I replaced the acid in the vessel with the manganese, and by subsequent trials made in the same way, and repeated after considerable intervals, I found that the residuum, when agitated, continued to evolve chlorine, and that during a very long time this effect was unabated.

Reflecting upon this unexpected phenomenon, I was naturally led to suppose that some muriate of manganese had been accidentally combined with the oxide, or that it was possibly rendered impure by the admixture of the muriates of iron or of copper, the presence of these metals being, as is well known, frequently detected in the ore of manganese.

In order, therefore, to separate the soluble muriates of these metals, if they should possibly be found to exist in combination with the manganese, I took a given weight of the latter substance, very finely powdered, and gradually affused upon it a large quantity of boiling water in a glass vessel, agitating it thoroughly. Having allowed the mixture perfectly to subside, I drained off the water, again renewed the affusion, and continued to repeat this ablution every day for three weeks successively, using sometimes cold and sometimes hot water. At the end of this time I tested the water which was then decanted from the washed manganese, by means of nitrate of silver, but was unable to detect the slightest appearance of precipitated chloride. I then poured upon the still moist manganese four times its weight of sulphuric acid, previously reduced, by the addition of distilled water, to the specific gravity 1.500. I allowed this mixture to stand for about four weeks, occasionally agitating it with a glass rod, and at the end of that time I found, when the dilute acid, now of a deep crimson or purple-red colour, was removed from the subsident manganese, and the latter agitated, that the most decisive evidence of the presence of chlorine was exhibited by the smell evolved from it. I have preserved this particular mixture carefully, and, although a period of more than twelve months has elapsed since the sulphuric acid was introduced to it, the evidence of chlorine, when the residuary manganese is agitated, appears rather to increase than to diminish.

Convinced, from the result of this experiment, as well as from previous observation, that chlorine must exist in the native black oxide of manganese, I added to a portion of the dilute solution of indigo in sulphuric acid a quantity of the crimson solution of the black oxide, (the supernatant fluid of the preceding experiment,) and I found, as I anticipated, the blue colour immediately *destroyed*, though not blanched; the hue of the indigo being replaced by the brownish green of the neutral sulphate of manganese.

The fact established by this experiment I have since found has been long known to chemists, although the striking evidence which it affords, of the presence of chlorine in the black oxide of manganese, they seem, most unaccountably, to have overlooked. Dr. John, in his elaborate *Essay on Manganese, and its Combinations*, (a translation of which appeared in the second and third volumes of the *Annals of Philosophy*,) refers to "the experiments of M. Giobert, first inserted in the *Mémoires des Sciences de Turin*, (Tom. v. p. 23, an. 1790 and 1791,) which he had produced as a proof that sulphuric acid, when treated with black oxide of manganese, attracts a portion of oxygen from the oxide, and is by that means converted into oxy-sulphuric acid, dissolving a portion of oxide, and thereby acquiring properties similar to oxy-muriatic acid, *as far as the destruction of colours is concerned.*"

Dr. Forkhammer, in his two papers on the subject of Manganese, published in the *Annals of Philosophy*, Vols. 16 and 17, to which I shall have further occasion to refer, repeats and confirms this statement. He says, "the solution of oxide of manganese in *muriatic acid*, even when no chlorine can be discovered by the smell, acts nearly in the same way as an aqueous solution of chlorine, metals being instantly dissolved by it. The *sulphuric solution* has the same properties, with respect to metals and vegetable substances, as the muriatic solution; which induced M. Giobert, about thirty years ago, to believe in the existence of an oxygenated sulphuric acid."

Having, in this manner, thoroughly satisfied myself of the presence of chlorine in the native black oxide of manganese, I was naturally led to inquire under what circumstances, or in what chemical state, it might be concluded to exist in the combination: that it should be either the muriate of the oxide, or the chloride of the metal, seemed in the highest degree improbable, from the single circumstance, that oxygen gas is abundantly obtained from it by the mere application of heat. A further consideration, however, of the last-mentioned fact ultimately suggested to me that view of the subject which, after the most minute and anxious investigation, I have been led to adopt, namely, that the chlorine combined with the black oxide is in the state of *chloric acid;* and that the native oxide is, at least in part, and probably in proportions, varying with the different specimens of the ore, a native chlorate of manganese.

The additional facts, and the reasoning by which I have been led to this conclusion, I shall now endeavour briefly to detail.

I am well aware that to obtain unquestionable proof of the existence of the chloric acid, in combination with the manganese, in this case will be attended with very great difficulty; indeed, as far as my knowledge of analysis extends, I entertain but little hope of that difficulty being speedily surmounted. One thing, however, will, I apprehend, be easily and satisfactorily established; namely, that while there are various well-authenticated facts within our present knowledge of manganese, which are altogether irreconcileable with the theory of its being a mere oxide, there is not one of these facts which is not compatible with the view now offered of its being, either wholly or in part, a combination of the chloric acid with a base of manganese.

Of these facts, that which will probably appear the most striking, is the spontaneous combustion of a mixture of oil with the black oxide. The circumstance is briefly mentioned by Dr. Ure, in the *Chemical Dictionary*, article MANGANESE, as follows :—

“The ore of manganese, which is known in Derbyshire by the name of black wad, is remarkable for its spontaneous inflammation with oil. If half a pound of it be dried before the fire, and afterwards suffered to cool for about an hour, and it be then loosely kneaded or mixed with two ounces of linseed oil, the whole, in something more than half an hour, becomes gradually hot, and at length bursts into flame. This effect wants explanation. It seems, in some measure, to resemble the inflammation of oils by the nitric acid."

The concluding observation in this extract is well deserving of attention. The close analogy which exists between the nitric and the chloric acids is well known: the proportions of combined oxygen are the same in both; the only difference is, that "one contains a proportion of azote, and the other a proportion of chlorine *." In its powers of supporting combustion, and more especially in its tendency to spontaneous inflammation, the chloric acid, however, far exceeds the nitric; as a proof of which, it is only necessary to instance the well-known fact, that a mixture of chlorate of potash and sulphur cannot safely be laid aside, as it is liable to sudden and spontaneous detonation. On the presumption, however, that the oxide of manganese, which is, in this instance, mixed with the oil, contains a portion of combined chloric acid, the heat and inflammation produced in the mixture are reasonably and satisfactorily accounted for.

Another fact in the natural history of manganese, equally inexplicable, according to the views hitherto entertained respecting that substance, was communicated to me by the late Mr. Wharmby, chemist-assistant to the Royal Dublin Society. He stated that, whilst in attendance upon the late Professor Higgins,—a man whose valuable and important labours in chemistry have been long and unjustly disregarded—he was engaged in the laboratory of the society in obtaining oxygen gas from a quantity of the black oxide of manganese intensely heated in an iron retort. He had, fortunately, retired some distance from the fire for a moment, when a loud explosion took place; the iron retort was broken into fragments, which were projected against the walls of the

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* Sir Humphry Davy on the analogies between the undecompounded substances.—*Quarterly Journal of Science,* vol. i. p. 287.

laboratory, breaking the windows, and otherwise producing considerable damage. Mr. Wharmby mentioned that an explosion, under precisely similar circumstances, had occurred in the Laboratory of the College of Maynooth; the particulars of which had been communicated to him by the gentleman who then filled the chair of chemistry in that institution. Indeed, of the authenticity of both circumstances, I believe no doubt can be entertained.

The most singular, however, of all the combinations of manganese is that under which we find it exhibited as the chamelion mineral. When black oxide of manganese and nitre, both reduced to powder, and mixed together, are thrown into a red hot crucible, the nitric acid, it has been assumed, is decomposed, and a compound of highly oxidized manganese with potash obtained. This theory, however, is inconsistent with the fact, that the same compound may be obtained by fusing the black oxide with mere caustic potash; the result, in either case, is the chamæleon mineral. A well known experiment, by Messrs. Chevillot and Edouard, has been supposed, in a great measure, to reconcile the *rationale* of these different processes. They assume that, in the combination of the oxide of manganese with the caustic potash, at an intense heat, oxygen gas is *absorbed;* and this opinion they sustain by the result of the experiment in question, where the oxide of manganese and the potash were exposed to the degree of heat necessary to effect the usual combination in an atmosphere of azote, the vessel in which the experiment was conducted being filled with nitrogen gas. They found, in this case, that no chamæleon was formed; and they infer that this was owing to the *absence* of oxygen: the *presence* of nitrogen would, however, seem to offer an equally rational explanation of the difficulty: at the degree of heat employed, the oxygen of the manganese would naturally combine with the nitrogen present, the oxide would be altogether decomposed, and nitrate of manganese might be expected to result. The experiment is, under these circumstances, quite unsatisfactory; to ensure perfect accuracy it should have been performed, if possible, *in vacuo.*

It would be a tedious and unprofitable task to recapitulate, however briefly, all that has been written upon the subject of this curious and interesting compound, or to enumerate the various and unsuccessful attempts which have been made to reconcile its unstable appearances with the generally-received theory of the oxides of manganese. The result of numerous and protracted experiments has fully satisfied me that the chlorate of manganese present in the native oxide *combines both with acids and with alkalis.* When subjected to the action of dilute sulphuric acid, it has been already shown that a *purple-red* solution is obtained. The combination with an alkali is effected by means of heat, in the manner which has been described for obtaining the cameleon mineral. When the resulting mass is dissolved in water, and allowed to stand for some time in contact with the air, *a solution of a purple-red colour* is in like manner obtained. Each of these solutions, after some time, deposits a brown oxide of manganese, and the colour disappears.

The solution formed by means of the acid is concentrated with difficulty, and crystals can in no case be obtained from it; when considerable heat is applied, oxygen gas, accompanied by the peculiar odour already mentioned as resulting from the solution of manganese in sulphuric acid, is evolved, and the colour is destroyed. This, however, is not the case with the alkaline combination: by cautious evaporation it is concentrated, and a red-coloured, regularly-crystallized salt obtained; this chemists consider to be a manganesite of the alkali, which is usually potash. When the crystals of this salt are heated in contact with hydrogen, they cause it to inflame. They detonate violently with phosphorus; set fire to sulphur, arsenic, and antimony, and to all other combustibles hitherto tried; and, as Dr. Thomson, in his recent valuable publication *, pointedly observes, "*they rival chlorate of potash itself in their powers of supporting combustion.*."

These properties of the manganeseous salt are altogether incompatible with all known facts, and every acknowledged theory, concerning the metallic oxides; but if, in conformity with the views here offered the salt resulting from the cameleon compound be taken as a triple compound of manganese, alkali, and chloric acid (chlorate of potash and manganese), the difficulty may be rationally solved, and the theory of its composition, as well as its inflammable agency, shown to be conformable to the other salts which are formed by the chloric acid.

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* First *Principles of Chemistry,* vol. i. p. 371.

As far as the destruction of vegetable colours can be received as evidence of the presence of chlorine, its existence in the manganeseous acid, and the salts formed by it, is already established. Dr. Forkhammer, in the papers previously alluded to, speaking of the crimson-red solution of the manganeseous salt, observes, that " it destroys the colour of litmus paper, and renders it brown, by depositing deutoxide of manganese; and *it would bleach quite as well as chlorine*, if its oxide were not coloured itself, and produced, by its deposition, a new colour*."

It would be easy to multiply these evidences of the presence of chlorine in the native black oxide, and in those salts into the composition of which it enters, and equally so to show the impossibility of reconciling the most important of the phenomena attendant both upon the oxide and the salts of manganese, with the circumstances which mark all the other simple combinations of oxygen with metals. I shall content myself with quoting from the paper of Dr. John, the most zealous of all the contributors to our knowledge of manganese, the following extract, which clearly shows the difficulty which he found to attend this part of the subject, and which led him to believe that the crimson colour and peculiar smell of the alkaline solution of the oxide, and which are equally obvious, when it is digested in sulphuric acid, were occasioned by the presence of a new and undescribed metal existing in combination with the native black oxide. He says, " I will here add a few words respecting a new metal which I have found in the Saxon grey ore of manganese †, the nature of which I hope soon to make evident.

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* *Annals of Philosophy*, Vol. xvi. p. 133.

† Dr. Forkhammer has observed upon the exact similarity which exists between the Saxon and the Devonshire ore of manganese; both contain a small

" This substance is distinguished from all bodies hitherto known by the following properties :—

" It appears to occur in the state of an acid, and it may be separated and volatilized, by the action of the mineral acids upon the ore. If the operation be performed in a distilling vessel, and an opportunity be given to the disengaged acid to combine with potash or soda-ley, a combination takes place, and a crimson solution is obtained. If this solution be mixed with another acid, and heat applied in a retort, the metallic acid is volatilized with a peculiar smell, and the residue is a combination of the acid employed with the alkali, without the smallest indication of any other substance, as far as I have been able to discover. The crimson-red solution I consider as a combination of a peculiar metallic acid with an alkali."

Whilst attentively considering the combinations of manganese, under the views and circumstances now detailed, I was forcibly struck with the analogy which appears to exist, in various instances, between the native black oxide of manganese and the peroxide of lead, and the result of the limited investigation which I have been enabled to give the subject, altogether tends to sustain the opinion, that this substance is also, in part, although in proportions not ascertained, and possibly very variable, a combination of chlorine, probably in the state of chloric acid, with an oxide of lead.

The peroxide of lead is generally obtained in combination with the red lead of commerce, from which it is separated by the action of nitric acid, which, dissolving the protoxide present, leaves the peroxide in the form of a brown or puce-coloured powder. The process by which red lead is prepared is universally known, and in arguing from all the acknowledged principles of chemical science, it must be admitted as utterly impossible that chlorine could, in that process, combine with the red lead produced. Experiment, however, proves that the fact is clearly otherwise. Let red lead, in the proportion of about half a pound, be

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admixture of copper, and both are found in the same geognostic position.—*Annals of Philosophy,* Vol. xvii, p, 50 placed in a deep glass-vessel, and concentrated sulphuric acid poured over it, to the extent of three or four pounds weight, allowing the mixture, after it has been well agitated, to remain at rest for some time. When the supernatant acid is drawn off, and the residuum agitated, the vapour of chlorine will be abundantly evolved, accompanied, in some instances, by a peculiarly nauseous smell, resembling that of arsenic. If the liquid be again replaced in the vessel, the examination of the residuum may be repeated at intervals, and for some days no material abatement in the evolution of chlorine will be perceived. It gradually decreases as the oxide is converted into sulphate of lead. When, by digesting red lead in nitric acid, the peroxide is separated in the first instance, similar evidence of the presence of chlorine may be obtained by the affusion of sulphuric acid; but it is requisite that the experiment be conducted on a large scale, in order that a considerable quantity of the peroxide may be obtained at once.

When my attention was first attracted to this subject, I was not aware that views of the constitution of the peroxide of lead, to a certain degree analogous with those I have learnt to adopt, had been previously entertained; and I found, with some surprise, on referring to Dr. Murray's *System of Chemistry*, that he had long ago expressed the opinion, that the peroxide of lead, as it is generally considered, is in all probability not a perfectly-defined oxide, but rather a sub-salt; although, with regard to the acid, which may be supposed to enter into the composition of the latter, he does not hazard a conjecture.

One point remains, to which I am desirous of reverting; namely, the production of the peroxide of lead, containing, as we have seen, a portion of combined chlorine, in the process by which red lead is prepared. The views which an attentive consideration of this part of the subject has suggested, I feel considerable hesitation in advancing. They refer to a question upon which the highest powers of the first philosophers of the age have been literally exhausted, and are opposed to views and opinions, the truth of which falls short only of demonstration.

That chlorine is present in the peroxide formed in red lead I have satisfied myself by the experiments already detailed, and which I have frequently repeated with minium obtained from different manufacturers. At the same time I have freely admitted, that it appears at first view impossible that chlorine could enter into combination with red lead during that process in which it is usually formed, and which consists in exposing the protoxide of lead to the flame of large coal fires, in a reverberatory furnace. If this subject be attentively considered, it will be evident that the oxide, during the continuance of the process, is exposed at a high temperature to the combined action of oxygen, nitrogen, carbon, and (though probably in a small degree) of hydrogen. The question hence naturally occurs, can it be possible that the chlorine, which the result proves to be present, is formed by the combination of all, or of any, of these gases. Its energetic affinity for hydrogen, as well as the result of all previous experiments on the subject, would seem to exclude that agent from its composition.

Whilst discussing this subject, it occurred to me to inquire how far the possible, but improbable, supposition, that chlorine is a compound of oxygen, nitrogen, and carbon would conform to its equivalent, or atomic weight, assuming it to be formed of a volume of each of these substances condensed into one volume. I acknowledge that I was surprised when, upon summing up the volumes of the three gases, I found the total accurately coincident with the atomic weight of chlorine:—1 vol. oxygen $16+1$ vol. nitrogen $14+1$ vol. carbon $6=36=$ the equivalent, or atom of chlorine.

Singular, however, as this coincidence appeared, I could not for a moment hesitate to consider it otherwise than as accidental; nor was I disposed to alter the latter opinion when, upon more attentive consideration, I found the elements in question, by an assumed division of the oxygen between the carbon and nitrogen, in the proportion of an atom to each, arrange themselves more reasonably as protoxide of nitrogen, $22+$protoxide of carbon $14=36$.

Unfounded as this additional view of the constitution of chlorine then appeared, the *possibility* of its truth struck me forcibly, when I recollected the close analogy which has been long observed to exist between chlorine and cyanogen, especially in the compounds which they form with metals—in the faculty which both possess, not only of forming peculiar and characteristic acids by their union with hydrogen, but also of decomposing water, by the agency of their energetic affinity for that element; and I was obliged to acknowledge that this analogy would be still more closely drawn, and would rest upon grounds still more natural and tenable, if we should hereafter be enabled correctly to assume that, while cyanogen is a compound, consisting of nitrogen (in excess) and carbon, chlorine is a similar compound, formed by the union of the protoxides of these two important substances.

Whilst attentively considering this view of the subject, it occurred to me that a very simple experiment ought to determine the question, and that if the constitution of chlorine should be in reality identical with a compound of the protoxides of nitrogen and carbon in equal proportions, it ought, if mixed with oxygen in excess, to form an inflammable compound, which would continue to burn when forced through a capillary tube, or would explode by contact of the electric spark, or of a lighted taper, and by the consequent combination with oxygen, be, in either case, resolved into equivalent volumes of nitric and carbonic acids.

The apparatus which I employed in making the experiment, intended to verify this view of the subject, was but rudely constructed. I placed in the bottom of a common Papin's digester a quantity of salt and black oxide of manganese, and into a tall earthen jar, which could be conveniently placed within the digester, I put a further considerable quantity of the oxide of manganese separately. I had previously fitted to the mouth of the digester a wooden cover, which, by the aid of a lute of common putty, could be rendered air-tight. In this lid there were two small holes, one of which, rather larger than the other, was closed at pleasure by a cork, and into the second I had inserted and luted the tube of a common tobacco-pipe. The vessel was so placed that the heat of a fire could be conveniently applied beneath it.

I now poured upon the manganese contained in the *inner* vessel a quantity of dilute sulphuric acid, previously heated to the temperature of boiling water, in order to facilitate the disengagement of oxygen gas, agitating the mixture thoroughly; and upon the salt and manganese I affused dilute acid at the ordinary temperature. The lid was then luted down, and heat applied to the vessel. I allowed the vapour, which was soon rapidly produced, to escape through the large opening in the cover, so as to expel the atmospheric air contained within the apparatus; and when this had continued for a few minutes, I closed the opening by inserting the cork fitted to it. The mixed gases were immediately forced through the tube with very considerable velocity; and upon applying a lighted paper to the extremity of it, I was gratified to see a pencil of lemon-coloured flame instantaneously kindled, which continued to burn for some time with a degree of brilliancy proportioned to the force with which the gases were expelled through the tube.

Over this flame I inverted a large glass globe, which was speedily filled with the vapour produced by the combustion; but upon bringing this vapour into contact with lime-water, I found that no evidence of the presence of carbonic acid was obtained. By further subsequent trials, it appeared that carbonic acid was not produced, but that the resulting gas was, in most respects, analogous to chloric acid, with a considerable mixture of euchlorine. Some hydrogen appeared to have been present, as an inconsiderable quantity of water was formed and deposited within the glass globe. This may be reasonably attributed to some muriatic acid which might have escaped the action of the oxide of manganese—a circumstance which frequently occurs in the commencement of the process for procuring chlorine.

In repeating this experiment a short time afterwards, common salt and sulphuric acid *only* were placed in the bottom of the external vessel, while the production of a larger proportion of oxygen was secured by increasing the quantity of the oxide of manganese in the interior one: in this case I found the product of the combustion the same, but the flame was brighter, and more considerable, and water was deposited in larger quantity.

On another occasion I procured the requisite excess of oxygen from nitre heated in an iron retort, but without any perceptible variation in the inflammable nature of the resulting compound. I have since found that if muriatic acid gas be passed through a series of tubes or vessels containing black oxide of manganese in excess, and exposed at the same time to heat, precisely similar effects are produced. The excess of oxygen mixing with the muriatic acid present forms an inflammable gas, which explodes with considerable force upon the application of a lighted taper, depositing water, and producing a vapour similar in every respect to that described as the result of the preceding experiment. I may here observe, that this mode of effecting the union of chlorine and oxygen by inflammation has not, as far as I can discover, been previously observed or practised.

Disappointed at finding that neither nitric nor carbonic acids, in a free state, resulted from this experiment, it occurred to me to inquire, could the new gas, supposing it to be formed by the chemical combination of the nitric and carbonic acids assumed to be present, be identical with chloric acid?—on referring the *possibility* of this view of the subject to the scale of equivalents, I was again surprised to find an accurate coincidence:—1 vol. nitric acid $54+1$ vol. carbonic acid $22=76=$the equivalent or atom of chloric acid.

Strange as this appeared, I felt myself, nevertheless, bound to admit that it might be merely the result of accident; and this opinion I should probably have continued to entertain, had I not been recently led to consider the close resemblance which exists between the common fulminating powder, produced by a mixture of nitrate of potash, carbonate of potash and sulphur, and the detonating compound formed of chlorate of potash and sulphur. The coincidence here, I apprehend, is too striking to be regarded as the effect of mere accident. When the fulminating powder is exposed to heat sufficient to produce perfect fusion, and its elementary particles are thus brought into intimate contact, it explodes with effects precisely similar to those which attend the inflammation of chlorate of potash mixed with sulphur, when exposed to an inferior degree of heat. If the views suggested by this inquiry into the constitution of chloric acid shall hereafter be verified, it will be seen that chlorate of potash is identical in its elements with the mixture of nitrate and carbonate of potash in the fulminating powder, save that the latter contains potash in excess. The usual formula for the preparation of it, three parts of nitre and two parts of carbonate of potash, forms a tolerable approximation to one atom, or equivalent, of each of these substances.

Various other facts, all tending to the same view of the subject, have been brought under my consideration. Not the least striking of these is due to the accurate and very remarkable observation of Dr. Ure, that in all the powerfully-fulminating compounds, nitrogen appears in every case to be an indispensable element: according to the received views of its composition, chlorate of potash, though one of the most powerful of detonating agents, would be an exception to a rule which seems otherwise invariable.

Another equally-curious coincidence is, the obvious analogy which would seem to exist between the elements which enter into the composition of gunpowder, and those which, under the views now detailed, would seem to impart to the mixture of chlorate of potash and sulphur, as well as to the fulminating powder, their detonating powers: although the proportions differ, the elementary substances would appear to be the same in all:—oxygen, nitrogen, and carbon; potash and sulphur.

I am very far from assuming that the facts now detailed, and the inferences drawn from them, are to be received as conclusive proofs of the accuracy of the views which they go to sustain. Those views can be verified only by the most decisive experiments, both analytical and synthetical. I am, unfortunately, not possessed, either of leisure, of the instruments of analysis, nor, at present, of health necessary for the task. If, however, the facts produced, both in regard to the oxides of manganese and of lead, and in reference to the possible constitution of chlorine, shall be found sufficient to attract public attention, I cannot doubt that the ingenuity of more practised analysts will, ere long, set both these questions satisfactorily at rest; either by reconciling the facts which have been detailed with received theories, or by verifying the views which I have ventured to offer.

Analogical proofs in science, such as those now submitted, like circumstantial evidence in the ordinary affairs of life, ought to be received with caution; both are, however, frequently indispensable to the attainment of truth. The analogical reasoning of Dr. Prout first accurately defined and conclusively settled our knowledge of the important doctrine of chemical equivalents; and although, in the language of the first philosopher of our age, "the substitution of analogy for fact is the bane of chemical philosophy," yet, as he freely and truly admits, it has its indisputable and "legitimate use in connecting facts together, and thereby guiding us to new experiments."

Art. II.—*Account of a New Hydraulic Machine, invented by* Mr. Joseph Eve.

[From a Correspondent.]

Sir,

I have not perceived in any of the scientific publications of the metropolis, amongst which yours holds such a distinguished rank, a notice of an hydraulic machine, invented by Mr. Joseph Eve, (for which a patent was obtained,) which is now exhibiting in the manufactory of Taylor and Jones, 11, Jewin Crescent, Cripplegate.

The machine I allude to, is a pump, with no valves to open and close, the moving parts being rotatory; their speed may, therefore, be increased at pleasure, and to an almost unlimited degree. The water pumped up is in proportion to that speed of the revolving parts, and to the force applied.

It is the most ingenious, and probably one of the most valuable inventions in which the late years have been so prolific. The few scientific unprejudiced and practical persons who have seen the machine, have given the most favourable opinion as to its utility. However, I will proceed to describe this pump, and its performance. In Plate II, A shows a front view; and B an interior view, after the end of the case with the cog-wheels is removed.

The principle is this. Two cylinders of equal diameter (3½ inches), and equal length (6 inches), move in close contact on axes or pivots, and revolve in opposite directions, in an outer case or box. These cylinders have each two wings, of three-fourths of an inch area, and two grooves, and as they revolve, the wing of one cylinder falls regularly into the groove of the other, alternately, and so on in rotation; and in order that the groove may present itself regularly to the wings of the opposite cylinder, and let them pass, cog-wheels, placed outside the case, are fixed to the axes or pivots, which project: these cog-wheels insure not only an even revolution of the two moving parts, but they communicate the power, which is applied by means of a handle, to the axis of a large toothed wheel gearing into one of the two cog-wheels.

The pump-case is placed upon a common pipe, descending down to the well 21 feet below. Two men, turning the handle, raise half a ton of water in three minutes with this small pump; which is allowed to be a most satisfactory result, considering that, as the first pump constructed on this principle, it has of course many imperfections.

By substituting an air-vessel with a hose and pipe, the machine becomes the most simple, strong, and effectual fire-engine. It may be converted into a water-wheel, where a small stream with a high fall of water exists, or be acted upon by steam as a rotatory steam-engine.

The advantages which it possesses over common pumps are too manifold and self-evident. The most conspicuous are,—a saving of power, on account of the friction being much less than in ordinary pumps. It requires no leathering, being made entirely of metal; it does not wear, as no parts touch or rub except the axes or pivots on their bearings. Its simplicity, strength, and elegance, and the ease with which it is turned into a fire-engine. the saving of room, and weight in pump-work, if applied to deep wells or shafts in mines, the advantages as a ship's pump, are peculiar, and too numerous here to enumerate; but so self-evident, that I shall not dwell longer on the subject.

I would invite you, and your scientific readers, to inspect the pump at Messrs. Taylor and Jones's, who, I am informed, are authorized to execute orders for pumps and engines on this principle.

I am, respectfully, Sir,
Your obedient servant,
A SUBSCRIBER AND ENGINEER.

Goswell Road, Nov. 22, 1826.

ART. III.—*Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by* William Thomas Brande, F.R.S., Professor of Chemistry in the Royal Institution, *&c.*

[Continued and concluded from p. 66.]

IX.

Phenomena of Earthquakes.

IN the preceding lectures, we have examined the materials which compose the strata or crust enveloping our planet; we have inquired into the order of their succession, into the nature of their component parts, into the evidences which they exhibit of the destruction of a former order of things, and into a few of the theories which have attempted to account for their origin and successive changes. We were thence led to consider the alterations which they are suffering by the slow action of the elements, and the causes and effects of their wearing down and decay. Thus have we found, that although the inanimate creation is in a state of apparent quiescence and repose, gradual operations and unheeded processes are everywhere carrying on, which, though not apparent to the superficial observer, display to the more inquisitive eye of science a series of wonderful and often unexpected causes, tending to the production of the most beneficial effects; and opening new evidence of the design and harmony that pervades the world. And if we, imperfect in our comprehension, and limited in our powers, dwelling upon an insignificant point in space, and scarcely attaining the use and perfection of our senses before we are summoned from the scene, are even now permitted to discern thus much in the economy of the creation, what, may we not naturally ask, must be the extent of causes and effects hitherto unknown and undiscovered; but preserved, as it were, to awaken the industry, and reward the toils, of future inquirers, and partly destined, perhaps, to be unfolded when the human race shall have risen in the system of being, to some unknown point of moral and intellectual perfection?

But, incessant as these changes that surround us are, they exhibit nature and her operations in a state of seeming inertness and repose, compared with the sudden and terrible devastation of the earthquake and volcano—to the consideration of which, the progress of our subject now brings us.

In entering upon this extensive and interesting inquiry, all particular descriptions and details must necessarily be avoided, as incompatible with the time now remaining for its discussion; so that I shall endeavour, from the history of earthquakes and volcanoes, to select, in the first place, a succinct account of their concomitant phenomena; we shall then attend to the changes which they effect in the country or strata which they invade, and thence, by a reference to the known physical and chemical habitudes and powers of bodies, we may endeavour to trace the agents concerned in their production, and thus arrive at their final cause. That the most active chemical powers of matter are called into action in the strata of the earth, is demonstrated by the frequent occurrence of hot springs of water thrown up from below, of a temperature greatly above the mean heat of the earth, and not uncommonly approaching, or even attaining, its boiling point. Hot springs are not uncommon in most parts of the world where volcanoes are unknown; but in their neighbourhood, and in volcanic districts, generally, they are common and abundant; and sometimes, as in the Geysers of Iceland, jets of boiling water are thrown up with explosions and concussions of the earth. But although we have these gradations in the temperature of springs, and though in one case the phenomena approximate, and in another recede from those of volcanoes, we are probably to look to one and the same cause for the production of these modified effects. Now, there are numerous substances, of which even the slow and gradual chemical action is attended by increase of temperature, and which, under certain circumstances, may become the source of most intense heat; and these, in the contact of water percolating the strata, would amply account for its elevated temperature: here, however, the *purity* of the water renders such an explanation inadmissible; and it has been supposed that water, thus originally heated, sends forth its vapours through the crevices of the rocks, and that the steam thus generated is condensed by and communicates heat to some pure and distant subterranean stream. Such at least is the only plausible theory of thermal springs; and the alternating jets, explosions, and concussions, that sometimes attend them, and that more especially characterize the boiling fountains of Iceland, have been referred to the escape of steam under compression, and to the mechanical impulse of its condensation, upon the water, which is well known to produce a peculiar cracking noise and explosion even upon a small scale. The waters of the Geysers, however, are far from pure; and, unlike those of Bath, Bristol, Buxton, and Matlock, in this country, they contain a large quantity of siliceous earth in solution, which they deposit, in the form of an incrustation or tufa, upon the surrounding basin; and some of the springs are contaminated by other substances, which are mechanically suspended in them, rather than chemically dissolved.

In the phenomena that attend earthquakes we have yet more decided evidence of the existence of subterranean fire and heat; and it will not be difficult to trace out their connexion with volcanoes, and their probable dependance upon one and the same cause.

Upon the atmospheric phenomena that are said to precede earthquakes, much dependance cannot be placed, and a low barometer, a very dry season, an electrical atmosphere, thunderstorms, and auroræ boreales, may, or may not, precede and attend an earthquake ; they are, at all events, very uncertain precursors. The serenity of the air immediately antecedent to the shock—the agitation of the ocean—and the disturbance of springs and fountains, are also very equivocal symptoms. In short, the only unvarying attendants upon the convulsion are those immediately dependant upon the chemically-acting substances, explosions and noises, attended by concussion more or less violent, by rifts and ruptures of the strata, by the occasional emission of hot air or even of flame, and by sulphureous and other fumes dependant upon the combustion of various bodies, to which the cause of the other symptoms is to be referred.

The noise that attends an earthquake is very differently described ; sometimes it is like the rumbling of distant thunder, or the blowing of a violent wind—a rattling like that of carriages over an uneven pavement is also commonly heard, and even explosions like those of large pieces of artillery—then the ground trembles, or its surface is heaved to and fro, sometimes in a perpendicular and sometimes in an horizontal direction, and great rifts and fissures are formed, from which water, mud, smoke, or fire issue, and which enlarge into chasms that swallow up the former surface ; large lakes are sometimes suddenly dried up—rivers alter their courses, or are lost—the sea itself is also greatly disturbed, and subject to changes no less remarkable than those occurring on land—shallows are often formed in deep water—mountains are even raised from the bottom of the deep, and currents of air, smoke, and various gases, and even of flame, are seen to issue from the water.

Among the numerous records of earthquakes I shall select that which destroyed Lisbon in 1755, as containing an epitome of all the disastrous effects and concomitant phenomena of these most awful and destructive visitations.

In the first place, it deserves notice that the approach of the great shock was gradual, and that, for three or four years antecedent to it, several considerable tremors of the earth had been perceived. On the 1st of November, at half-past nine in the morning, a rattling noise, like that of distant thunder, immediately preceded several short and quick vibrations, which shook the very foundations of the city, so that many houses instantly fell; there was then a short pause, and a swinging motion followed, which laid the greater part of the houses in ruins, and destroyed thousands of the inhabitants in less than six minutes. The bed of the Tagus was in many places elevated—ships were driven from their moorings—and the pier, upon which were several hundred persons, sunk to an unfathomable depth. The bar of the river was at one time quite dry, but suddenly the sea rolled in, and covered it with sixty feet of water. At Colares, twenty miles from Lisbon, the earth was also agitated and rent asunder, emitting smoke and flame. St. Ubes, twenty miles south of Lisbon, was swallowed up, and the sea everywhere inundated the coast, and threatened the destruction of Cadiz. At Gibraltar there were the same extraordinary ebbings and flowings of the tide, and there the most violent shock was felt about an hour later than at Lisbon, and at about the same time as at Madrid, where persons walking the streets were turned sick and giddy, but where little other mischief ensued. This same earthquake destroyed Algiers, a great part of the city of Morocco, near which from eight to ten thousand people were engulphed, and, the earth again closing, were seen no more. Various towns upon the African coast of the Mediterranean also suffered. At Madeira the shock was felt nearly at the same time as at Lisbon. The sea rose fifteen feet above high-water mark, and did great damage. Over the whole of Europe the effects of this earthquake were more or less perceived, and when the violence and extent of the mischief at the immediate scene of its action is considered, one is rather surprised that the damage should not have extended beyond the limits that have been mentioned.

We may refer to the various printed accounts of this and other earthquakes for further details as to particulars, the evidence before us being amply sufficient to prove that the phenomena are referrible to some sudden and powerful chemical actions going on at great depths below the earth's surface, while the flame, smoke, and other appearances common to volcanoes, show the connexion of the two effects, and lead us to refer them to one cause; the explosions and agitations of the earthquake being analogous to those which might be expected to occur from volcanic eruptions wanting vent, and exerting their force upon the solid vaulting of the superincumbent strata.

But the immediate connexion between earthquakes and volcanoes may often be very satisfactorily traced. Pallas says, that on the 5th September, 1790, a subterraneous rumbling seemed to issue from the sea of Azof, and then, with an explosion like that of a piece of ordnance, an island emerged from water thirty feet deep; it was of a conical form, and ejected mud, stones, smoke, and flame, and the surrounding country was severely shaken at intervals during the day by repeated shocks of an earthquake.

In January and June, 1783, Iceland was severely agitated by earthquake, and a dreadful volcanic eruption burst forth in the sea, at its south-west extremity.

In June, 1811, Captain Tillard, on approaching the island of St. Michael, observed, rising in the horizon, two or three columns of smoke, which proved to issue from a marine volcano, about a mile off the north-west end of the island, whither he proceeded, more nearly to inspect the phenomena. "Imagine," he says, "an immense body of smoke rising from the sea, the surface of which was marked by the silver rippling ~~of the~~ waves, occasioned by the slight and steady breezes incidental to those climates in summer. In a quiescent state, it had the appearance of a circular cloud, revolving on the water like a horizontal wheel, in various and irregular involutions, expanding itself gradually on the lee side, when suddenly a column of the blackest cinders, ashes, and stones would shoot up in the form of a spire, rapidly succeeded by others, each acquiring greater velocity, and breaking into various branches resembling a group of pines, these again forming themselves into festoons of white feathery smoke. During these bursts the most vivid flashes of lightning continually issued from the densest part of the volcano, and the columns of smoke rolled off in large masses of fleecy clouds, gradually expanding themselves before the wind, in a direction nearly horizontal, and drawing up a quantity of water-spouts, which formed a striking addition to the scene. In less than an hour a peak was visible, and in three hours from the time of our arrival, the volcano then being four hours old, a crater was formed, twenty feet high, and from four to five hundred feet diameter."

The account then goes on to state, that the eruptions were attended with a noise like the firing of cannon and of musketry intermixed, as also with shocks of earthquakes, sufficient to throw down a large part of the cliff on which the observers stood. Captain Tillard afterwards visited the volcanic island: it was eighty yards high; its crater, upon the level of the sea, was full of boiling water; it was about a mile in circumference, composed of porous cinders and masses of stone.

This incomplete abstract of Captain Tillard's paper is sufficient for my present purpose, that of connecting the phenomena of boiling springs and of earthquakes with those of volcanoes; and taken in connexion with the similar histories to which I have adverted, and with others that are upon record, but which I have not thought it necessary to quote, seem to show that, if we can discover the efficient cause of volcanoes, that of earthquakes and their concomitants may be referred to the same source. I shall therefore now briefly advert to the history of volcanoes, and thence endeavour to trace, upon analogical and experimental data, the probable causes of their various effects, and the sources of their tremendous powers of destruction.

Volcanoes are not confined to any district or part of our globe, either geographical or geological. The Italian territory, and the southern extremity of the Mediterranean, present the most awful monuments of their devastation. In America the whole chain of the Andes is subject to the most terrible earthquakes, and from Cotopaxi to the South Sea, there are, according to Humboldt, more than forty craters in constant activity. Extinct volcanoes are by no means uncommon, and in the Vicentine territory there are rocks graduating from distinct lava into those varieties of basalt usually called amygdaloid. On the contrary, new volcanoes are exceedingly rare, and there are not more than two or three instances of these eruptions having broken out in new places; but then we are to remember that, as more than two-thirds of the globe are covered by water, explosions may take place in the depths of the ocean, and may not produce any observable effect, though they are sometimes announced by extraordinary perturbations of the waves, high swells, and even sometimes a volcanic mountain is thrust from the bottom of the sea, as happened, in the instance that I have already narrated, off the Azores, in 1811, in water sixty fathoms deep; indeed the soil and aspect of all those fertile islands, as well as of many of the West Indian isles, seems to indicate that they are of similar origin, and frequently are found to consist entirely of coral beds and volcanic formations.

The truly volcanic countries are remarkable for the conical form of their hills; the Bay of Naples furnishes a fine panoramic view of their general aspect, and one of the most prominent of the hills which surround it was raised, in the year 1538, in a single night, to the height of one thousand feet, and three miles in circumference. It may serve as an instance of the force and power of the volcanic elements.

In these countries the singular transmutations of surface connected with the history of their formation, often deserve notice. Near Naples, for instance, the Lake of Agnano is evidently the crater of an ancient volcano. The cone of the Solfaterra is seen near it, and the Grotta del Cane upon its edge. There is, indeed, no country better adapted to illustrate the power of volcanic fires than the south of Italy, nor have the phenomena connected with them been anywhere more completely observed or investigated. Of the antiquity of Vesuvius, and of the prodigious quantity of matter which it has erupted, a good idea may be formed from the inspection of the subterranean ruins of Pompeii and Herculaneum, both of which were buried by an eruption of the mountain, in the first century of the Christian era. The former city is now twelve feet, and the latter about one hundred feet below the present surface; but these cities are themselves founded upon beds of lava. Thus we see that these tremendous agents have at one time destroyed countries of ancient formation, and at another created new territories, whose subsequent fertility has, as it were, counterbalanced the desolation that accompanied their production. It need hardly be mentioned, that the most delightful and celebrated portions of Italy have been either formed or modified by the agency of fire. The rock of the Capitol, which Roman vanity called *eternal*, is the tottering edge of a crater. The *Campagna Felice* is the obvious production of successive lavas, and owes its exuberant fertility to repeated showers of volcanic ashes.

Etna and its vicinity also present a magnificent and characteristic specimen of a volcanic country; and though so frequently destroyed by rivers of red-hot lava, and overwhelmed by torrents of fetid mud, and showers of sulphureous dust, its neighbourhood is covered with the most prolific vegetation and majestic forests. The height of this mountain is about ten thousand feet, and the ascent from Catania carries the traveller through various zones of temperature, until he reaches the black forest of firs and pines, above which the rugged, barren, snowy precipices announce his approach to the summit. The crater of this mountain is upon the most magnificent scale. It is about two miles in circumference, and bounded by black and tottering walls, in many places perpendicular, and upwards of fifteen hundred feet in height*.

The phenomena of volcanic eruptions of any magnitude have often been but imperfectly described, because the authors who have witnessed them have not been guided in their observations by scientific views: they have generally been overpowered by the tremendous and extraordinary phenomena that they have witnessed, and have rather endeavoured to transfuse into the minds of their readers the horrors and devastation of the scene, than to describe it with that minuteness which their opportunities would sometimes have afforded. Breislac is one of the most intelligent writers upon this subject, and both he and Sir William Hamilton have described the dreadful eruption of Vesuvius which destroyed

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* For a further description of Etna, see Vol. XIX. of this Journal.

Torre del Greco, in 1794. As these accounts are well known, I shall merely advert to such parts as tend to illustrate the nature of the substances ejected, and to throw light upon the ultimate point of our inquiry, namely, the cause of volcanic fires.

On the evening of the 15th of June, 1794, after the neighbourhood had experienced several preliminary earthquakes, the base of the crater opened on the west, and ejected a torrent of lava, which in six hours flowed three miles, and, after destroying the town of Torre del Greco, ran into the sea, forming, according to Sir W. Hamilton, a promontory upwards of twelve hundred feet in breadth, and six hundred in length; the lava contained augite and crystals of mica. On the 16th, the lava made its escape on the east side of the crater, and the mouth of the volcano continued to eject ashes for four days, which covered the adjacent country, and so obscured the atmosphere, that at Caserta, which is more than ten miles from Vesuvius, they were obliged to use torches at midday, and the gloom was only broken by frequent and vivid flashes of lightning, which played about the summit of the mountain. On the 20th, the ashes ceased to fall, and showers of red-hot stones were ejected, accompanied by a constant succession of dense vapours; and rain mixing with the loose ashes, formed impetuous and destructive torrents of mud; while carbonic acid and nitrogen gases, with small portions of sulphurous acid, contaminated the surrounding atmosphere, and diffusing themselves over particular districts of the country, were equally fatal to animal and vegetable life.

I might quote other cases and other accounts of volcanic eruptions, but it would be found that, with few exceptions, they agree in their main and principal points,—in their preliminary phenomena, in the nature and ejection of the lava, in the explosions of hydrogen gas and aqueous vapour, and in the production of mephitic gas; and such being uniformly the results, we may safely refer to them for a clue to the phenomena.

One of the earliest hypotheses to account for volcanic eruptions is that which attributes them to the eructations of a perpetual central fire, to which, however, the nature of the lava, the method of its projection, and, above all, the known laws of the communication of heat, are insurmountably opposed. The sudden evolution of steam has also frequently been resorted to; indeed the power of this agent did not escape the genius of the ancients, for Lucretius, in his description of Etna, clearly ascribes the phenomena to its effects. They have also been referred to the ignition of beds of coal, and Werner supposed that the fire thus produced fused the circumjacent rocks, and formed lava. Others have called sulphur, pyrites, petroleum, and bitumen to their aid, but have sought in vain for the necessary supply of oxygen, without which these combustibles could not perform their required part; and, indeed, if we grant an unlimited supply of that element, the projectile force—the vapour, still remains to be accounted for. Others have imagined a great depôt of electric matter, pent up in certain submarine and subterranean caverns, and occasionally sallying forth to fuse and blow up the surrounding materials.

It appears to me that, among the most plausible theories of volcanoes, is that suggested by Sir H. Davy, soon after he had discovered the nature of the earthy and alkaline bodies. Indeed, it enables us, in most cases, upon just principles of sound analogy, to explain their origin; for lava consists of earthy and alkaline bodies, ejected in intense ignition, and it is associated with vapour, with explosions of hydrogen gas, with the production of nitrogen, and, in short, there is every concomitant circumstance to lead to the conclusion, that there exist, in the bowels of the earth, masses of those highly inflammable metallic bodies constituting the bases of the earths and alkalies, and these and water are essential requisites for the production of the phenomena that precede, accompany, and follow the eruption of volcanoes; they may be referred to as accounting for the earthquakes, the explosions, and the gaseous products; and they are the *only* agents with which we are acquainted, capable of fulfilling all the requisites. How or where these bodies exist—at what depths—in what quantity, and how accessible to water, are questions that we cannot solve, and which, therefore, I shall not discuss, more especially as they are not necessarily connected with the main question before us, which merely requires their existence, and the presence of water; and it is curious that water is always found connected with volcanoes. Vesuvius, Etna, and Hecla are upon the verge of the sea; and in the vicinity of the burning mountains of the Cordilleras there are lakes; and it has been observed that springs and lakes suddenly dry up previous to the active eruption of a volcano *.

I have before adverted to the analogy that subsists between basalt and lava, and have endeavoured to show, that the appearances of the rocks and strata that are invaded by basaltic veins, accord, in some respects, with the igneous theory of their origin. I have also noticed some singular facts connected with granitic and porphyritic veins, and have traced analogies which may serve as theoretical links to connect them with the basaltic formations. How far, then, are we able to trace a resemblance between these products and their effects, with those of ordinary and now extinct volcanoes? Although it must be allowed that, among modern lavas, we have nothing strictly like that crystalline aggregate of quartz, felspar, and mica, which we call granite, we have among them substances so closely resembling certain basaltic rocks, that it is difficult either chemically or mineralogically to distinguish between them; and in lava, crystals of mica are sometimes found, not accidentally blended with, but actually formed in it, and of substances differing rather in form than in composition from felspar. Is it possible, therefore, that certain appearances exhibited by the strata of our globe, not referrible to aqueous detritus, and evidently, indeed, unconnected with any probable agency of water, may have arisen from the more superficial existence of those inflammable metallic bodies. That their combustion and fusion may have left behind them those equivocal rocks, and that the limited duration and non-recurrence of such phenomena, may be ascribed to the destruction of the agents which have produced them, and to the non-existence of those substances near the surface, which the phenomena of volcanoes induce us to believe are still, though probably sparingly, disseminated in particular situations?

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* A very different hypothesis upon the subject of volcanoes has been published, since the above was written, by Mr. Scrope, to which the reader is referred.

Art. IV.—*Observations upon the present Mode of Paving, with a Plan for its Improvement.*

[Communicated by the Author.]

Having observed that the present system of paving works badly, and imagining that it admits of easy and effectual remedy, by which economy and general convenience might be durably attained, I would first point out the following as its chief

Imperfections.

The *nature* and *state* of the soil in which the paving stones are placed.

The wedge-like form of the stones.

The great distances between them at their lower surface.

The frequent disturbance of them for the repair of pipes and sewers.

On a comparison of these imperfections I would observe, that while the present soil is continued as a substratum so readily softened by water, and dug up loosely as though to facilitate the stones driving into it at irregular depths, neither the biangular, the block, nor any other pavement, ancient or modern, will prevent its rapidly acquiring an uneven and muddy surface, as a permanent support can only be obtained either perpendicularly by a substratum nearly unyielding, or diagonally by buttresses; the latter being impracticable from the expense it would involve, the cellars it would force in, and the old houses it would tumble over our heads, leaves the alternative of obtaining a good substratum of road on which to place the pavement; which I propose to obtain by the following

Plan.

Let the stones be for the present removed. The soil, to the depth of eight inches, sifted, the fine portion sent off altogether, the coarse retained for the road; but, before spreading, the subsoil should be dug and raked, to bring the coarse material to the surface, which should be covered by that obtained from screening, and a surface added of about three inches of such of the present pavement, properly broken up, as are totally unfit for tolerably good pavement. When sufficiently worn, I would add three inches of the same material: so far it will be seen I propose to form a macadamized road, on which, as soon as it had acquired by raking and wear an uniform and solid surface, I would, without scraping it, lay paving-stones of equal depth and width, and as square as they could be obtained, consistent with economy, introducing the large end downwards where there was any approach to a wedge-like form, always remembering to place the lower surfaces of the stones in close contact, both at sides and ends, and that no two joints should follow in continuity. I would then ram into the interstices such thin chippings of granite as might fit them, and wash in scrapings from a granite road, or any cheap body of more cementing properties, and which would be as little affected by changes of temperature. This plan would require for the sub-road the bulk of the present pavement, leaving a few of those most uniform in depth, and least wedge-like in form, to be introduced, the large end downwards, in a length of street unmixed with the new pavement, yet treated in the same manner.

Pipes should be laid as close to the curb as possible, where the traffic is least; and whenever the ground is filled in after repair of pipes or sewers, it should be well rammed throughout its depth; and no soil that would be readily acted on by water should be allowed within eighteen inches of the surface.

It will be allowed that a wedge, either of stone or wood, is adapted to drive into soft ground; and though a pavior may force it pretty hard, it will not stop there; for if the maximum of force from a pavior's rammer be equivalent to five cwt., (a large estimate,) it must be expected that a wheel, conveying a weight of a ton and a half, must needs drive further; and more especially where, by holes already formed, the weight and impetus will frequently amount to more than two tons from a single wheel.

Common earth and rubbish readily mix with water; thus mud must be formed under the pavement in wet weather, which, passing up through the needlessly wide interstices, occasions a rapid accumulation of mud on the surface, and makes room for the descending stones, some of which, being on softer ground, or more wedge-like in form, or smaller, or placed originally lower than those next to them, sink more; a reservoir is thus formed for the rain, which sinking into the ground and forming mud, the wheels which pass with increased force into such holes enlarge and deepen them. The deeper the hole, the greater the force of the descending wheel, and the longer the continuance of the reservoir to mix with the earth; so that the evil increases in a two-fold ratio.

This plan would, I think, realize the following

Advantages.

The sub-soil or road would no longer yield mud, nor allow the wider and flatter surfaces of the stones to pass into its hard and level surface, and form holes in the pavement.

Horses and carriages would all be adequate to more work, with less expenditure of strength and soundness. (Indeed, the great mass of suffering endured by the inflamed joints of, perhaps, every hackney coach-horse, should of itself induce humane persons to promote some reasonable plan to remove the cause of it; and the quantum of suffering borne by other carriage and saddle horses must be very considerable.)

The inconvenience and danger to riders and their horses would be much diminished.

Commerce, and the various objects of passengers of all descriptions, would seldom be interrupted by the mending of pavements.

Even the footpaths, freed from the abundant supply of mud from the carriage-road, would be much cleaner.

The sewers would, probably, never need cleaning, and being permanently more free from mud, would contribute more even than they now do to the health of the inhabitants.

I by no means think that this plan would produce a perfect road; but I believe that it would be more permanently level, durable, clean, and economical, than has ever been seen in any much-worn street of this metropolis.

I have calculated that permanently-good paving in London, Westminster, and Southwark, would save, in the wear of horses and vehicles of all descriptions, not less than one hundred and forty thousand pounds per annum; besides that fewer horses, and somewhat lighter carriages, would be adequate to the work required. Should this become apparent, the interest of brewers, coach-masters, coal-merchants, and the poor hackney-coachmen, would be materially served; and every humane person rejoice at the great reduction in the sufferings of the most noble of quadrupeds.

I wish also to suggest the following observations on the adaptation of Macadam's roads for the chief thoroughfares of this metropolis.

It would seem that, for a road not extremely used, this plan is far preferable to any other yet introduced; but such is the rapid wear, by pulverization, of granite and all other brittle substances, and so slowly do they yield to mere friction, that I apprehend where the road would wear ten inches in a year, the well-paved street would not yield more than one fifth of an inch in the same period, after the first wear of the sharp edges of the stones, which with close joints would be very trifling.

A macadamized road, if much used, requires such constant watering in dry weather to keep it together, and in wet wears so rapidly, especially on the level streets, and those running east and west, thereby being shaded by the houses to the south, that a constant expense is entailed either way.

If any doubt the great difference in the destructive properties of pulverization and friction on brittle substances, it may be well to notice the patent illuminators for ships' decks, which last generally for many years unaltered, except by some scratches on their upper surface; yet were any one of them to be broken into twenty pieces, so as to render them, by their size and shape, liable to pulverization, there is not one of them but would be ground to powder by a thousandth part of what some of them have borne.

GEORGE KNIGHT.

Norway Wharf, Limehouse.

Art. V.—*On* Mr. Tulley's *thick Aplanatic Object-Glasses, for Diverging Rays; with an Account of a few Microscopic Test Objects.* *By* C. R. Goring, M.D.

[Communicated by the Author.]

When we abandon the simplicity of single lenses for a compound instrument, in which a magnified image or picture of an object must be viewed instead of the object itself, a great obstacle to perfect vision arises, because the image must be formed, if possible, as perfect as nature itself; which can only be effected by an object-metal of perfect figure, or a compound lens free from aberration: so that it becomes as arduous a task, at least, to form a perfect compound microscope, (or, strictly speaking, an *engiscope*,) to act upon near objects and diverging rays as a perfect telescope. Ever since the invention of achromatic object-glasses for telescopes, opticians have been fully aware of the propriety of placing those of microscopes on the same footing: but the difficulty of the enterprise has hitherto prevented its execution, though many theorems on the subject have been given by various mathematicians: however, this great *desideratum* has at length been happily accomplished by Mr. W. Tulley, in this country, and by the elder Vincent Chevalier, No. 69, Quai de l'Horloge, at Paris. Before their time many vain and fruitless attempts were made in England, and probably also on the continent, to produce an achromatic object-glass for a compound microscope*.

It cannot, I think, prove uninteresting to amateurs of the microscope, to give an account of the circumstances which led to the formation of the first *effective* aplanatic lens for diverging rays. The honour of the discovery and execution of its curves belongs entirely to Mr. W. Tulley: yet the public will, I hope, pardon my egotism, when I assert that, without my agency, it would never have been made; for at my instigation, and at my expense, was this valuable present to the observer of nature produced. I pique myself upon having been Mr. Tulley's coadjutor in this affair; inasmuch as it was possible for a mere looker on and adviser to be.

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* Achromatic glasses—how far this invention is applicable to the improvement of microscopes has not yet been ascertained; and, indeed, from some few trials made, there is reason for supposing they cannot be successfully applied to microscopes with high powers; so that this improvement is yet a *desideratum* in the construction of microscopes, and they may be considered as being yet far from their ultimate degree of perfection.—Adams *on the Microscope*, pp. 48, 49.

I was always addicted to the use of single magnifiers, in my microscopical studies, which I felt to be far superior to all other microscopes in use; yet I always had a lurking fondness for compound instruments, with whose large field of view, and facilities for the illumination of opaque objects, as well as the application of micrometers and graphic eye-pieces, &c.* I could not help being much smitten; though I was fully sensible of their great imperfections in point of perfect vision: I thought it would be a glorious improvement if they could be rendered equal in point of optical performance to single microscopes, because they would then have a most decided superiority over the latter, from the accommodations they afford for the observation of every species of objects. The eyes of very few individuals are, by nature, gifted with such energy as to endure the use of deep single lenses with impunity for any length of time; and even the most favoured observers must allow that these show so small a portion of an object, and are so disagreeable to use from their very minute aperture, that it is a downright labour to employ them, even when we have been habituated to them for years.

Who would not prefer the easy quiet vision of a compound instrument, in which we may obtain abundance of working power, without looking through an eye-glass of shorter focus than half an inch, with its large field of view, and commodious apparatus and adaptations for every purpose, provided vision can be accomplished with the same accuracy, brilliance, and distinctness, *power for power*, as in the simple single microscope, which has hitherto been justly preferred by the most distinguished naturalists, with all its inconveniences, to every other kind of instrument?

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* Mr. Keir, the engineer, has one of Mr. T.'s object-glasses of 0.4 focus, to which a Troughtonian micrometer is attached, with which Mr. K. has succeeded in measuring accurately the $\frac{1}{100000}$ part of a linear inch. I do not adduce this as an instance of the superiority of the object-glass he used (because I think the same might be effected with any common compound microscope, by selecting an object visible with a very reduced aperture, so that the necessary distinctness might be obtained), but I am quite confident no such quantity will ever be measured with a single microscope; first, because no micrometer can be made for such an instrument, possessing a scale of a linear inch divided into 100,000 parts; and, secondly, because the object and the micrometer can never be seen distinctly at once, being necessarily at different distances from the magnifier. Mr. K.'s objects are usually the diameters of the constituent fibres which frequently form a single cobweb, and the distances at which they are separated from each other.

I found that common compound microscopes were totally unable to discriminate a certain class of objects which I had met with, (but which I saw with great facility with single lenses of adequate apertures,) unless I began by making an object-glass of the magnifier which, *by itself*, had the power of demonstrating the object under examination: so that, by attaching a compound body to it, I was compelled to make use of a power quadruple at least of what was really necessary, and after all did not see so well as before. I was thus led to examine into the defects of these instruments, and found their incapacity to be owing to the very small angles of aperture which all the useful working object-glasses possessed; whose aberration is conquered and rendered insensible by the reduction of their diameter, till they can " fling but half an image on the straining eye," or in many cases no image at all; in short they appeared to me to be much in the same predicament as the object-glasses of refracting telescopes were before they were rendered achromatic, (viz.) admitting of only a very small angle of aperture, and a very shallow magnifier, applied to the image in consequence of its want of light and distinctness; there being no alternative between blinding the instrument by a very small aperture, or letting in a volume of fog and prismatic colour by enlarging it. There is a property requisite in microscopes as in telescopes different both from distinctness and magnifying-power; which may be resolved into *penetrating-power*, and is the result of a certain breadth in the visual pencil of rays, (which is determined by the aperture of the object-glass or metal,) in proportion to the amplifying power made use of. It is evident that microscopes having to act upon diverging rays, the quantity or magnitude of their aperture must be reckoned always with regard to the focus of the object-glass or single-magnifier, and must be expressed by the *angle* formed by the diameter of the said aperture with the radiant point; in telescopes the actual aperture gives the penetrating-power without reference to the focus of the object-glass, because they deal with parallel, or nearly parallel rays. It is perfectly well known that, however correct a telescope may be, and however high its power, it will not be effective upon a certain class of objects, unless it also possesses the requisite penetrating-power: thus clusters of stars and nebulæ and all faint and distant objects can only be seen with very large apertures. There is a set of microscopic objects which bear a certain analogy to these, and can only be seen by microscopes, having large angles of aperture, whether single or compound, (viz.) the fine engraved lines or furrows on the feathers of the whole tribe of moths and butterflies, and on the scales covering many of the *curculios* and brilliant beetles: one of these objects is represented in the plate (Fig. 1.) and is there sufficiently identified: it is, I believe, a feather from the wing of the Menelaüs butterfly from South America; it is about $\frac{8}{1000}$ inch long and $\frac{7}{1000}$ broad, of a light-blue colour, and when viewed by a microscope of adequate penetrating-power under the necessary kind of illumination, is completely resolved into lines running longitudinally from end to end about $\frac{1}{11000}$ or $\frac{1}{12000}$th of an inch distant from each other: cross striæ are also visible as seen in the drawing, which are the most difficult points about it; the straight furrows of the specimen in question may be considered as easily demonstrable objects of the sort. The seven little circles contain portions of the superficies of the feather as exhibited by a compound microscope having an achromatic object-glass of .933 solar focus, and a power of 160 (equal to that of a single lens of $\frac{1}{20}$ inch focus) with the different apertures designated in the plate. As a transparent object my observations upon it run thus:—

1*st Observation.*

0.1 *of aperture.*—I see the outline of the object well defined: it appears dark and of the same tint all over; there is no vestige of lines visible.

(*Obs.* 2.)

0.2.—The feather is now more transparent: faint scratches are visible here and there; I should suspect a proper aperture would exhibit lines all over it.

(*Obs.* 3.)

0.26.—Patches of crooked, irregular marks, or ribs, now become perceptible, the feather exhibits that mottled appearance common to all such objects when seen with a defective aperture.

(*Obs.* 4.)

0.28.—I can now clearly perceive lines, but they are *dotted* rather than traced, and their continuity is broken and interrupted: this is the phenomenon shown by the whole tribe of these objects when seen with angles of aperture, nearly, but not quite sufficient to resolve the lines.

(*Obs.* 5.)

0.3.—The furrows are now seen completely over the whole surface, but still ragged and irregular; they are rather dotted than fairly drawn out: the space between them, also, is not so transparent as it should be.

(*Obs.* 5.)

0.4.—The lines are now completely developed, their image is perfect, the space between them is abundantly transparent; in order to see them to the greatest advantage it is requisite that the stand of the microscope should be so constructed as to admit of the flame of a candle being placed immediately behind the stage without the interposition of any condensing lenses or mirrors. To procure vision of the object, let it be placed vertically in the field of view; then move the candle out of the axis of the microscope, so that its rays may flash obliquely and transversely across the lines. In the plate (Fig. 2.) I have endeavoured to give an elevation of the relative positions of the candle and object; and at Fig. 3. is a plan, or section of the same on the line AB: these lines are also visible as opaque objects, with .3 of aperture, and the same management of the illumination as when seen by intercepted light. A single magnifier of $\frac{1}{10}$ focus, and about $\frac{1}{25}$ of an inch of aperture, shows them also; they are moreover visible with a compound aplanatic lens of Mr. Herschel's construction, of $\frac{1}{6}$ of an inch focus, and $\frac{1}{15}$ of an inch aperture: the power being 48, (rating by a standard of sight of eight inches,) which is the lowest power I have ever seen them with. They are just visible also with the .933 achromatic object-glass: and with the same power, they are totally invisible with common compound microscopes, having object-glasses of 1 inch, $\frac{1}{2}$, or $\frac{1}{4}$ inch focus, with the usual apertures.

(*Obs.* 6.)

0.5.—On looking with great attention with this aperture, having the candle extremely near to the object, turned one quarter round from the vertical position in which the horizontal lines are most evident, traces of punctuations or cross striæ became sensible, though by no means sufficiently verified to produce conviction of their existence. An achromatic object-glass of .2 focus, with a power of 240, is requisite to develop them fully; they seem to pervade the whole structure of the feather, but are scarcely ever to be seen over its whole surface at once, but appear in patches here and there, as represented at fig. 1. Mr. J. Cuthbert discovered these cross striæ with an Amician microscope of his own construction, and pointed them out to me; previous to which time they had escaped my observation, though I had examined the feather hundreds of times with my achromatics and with the deepest single lenses: both of which, nevertheless, show them very well under the necessary favourable circumstances, as before stated.

I have been thus particular concerning this test-object, because, in fact, it laid the foundation of the aplanatic object-glasses, and caused me to instigate Mr. W. Tulley to reform the common compound microscope, and in the following manner: Having, for experiment's sake, enlarged the aperture of a $\frac{1}{2}$ inch object-glass of a compound microscope from $\frac{1}{20}$ to $\frac{1}{4}$ inch, and applied a deep eye-glass to the body, I found I could see something of the lines in question, only they were exceedingly confused and indistinct on account of the chromatic and spherical aberration; it therefore occurred to me that if this could be removed without reducing the aperture, the said object would be seen in perfection. About the autumn of 1823, I applied therefore to Mr. Tulley, and requested him to make me an achromatic object-glass for diverging rays of as short a focus as possible, putting him in possession at the same time of the information I had acquired from my experiments on microscopes and test objects. I found that he had a passion for the subject, and had already been employed by Mr. Troughton to make a dozen achromatics of 1 inch focus, for his astronomical circles, in May, 1807. This attempt, however, proved completely abortive, the lenses being justly rejected by Mr. Troughton as worse than the common object-glasses: of the correctness of which sentence I was perfectly satisfied by an inspection of one of them which was put into my hands: nothing, indeed, is so easy as to make them after this fashion: for, in the same proportion that they may be made to surpass ordinary ones, may they be made to fall short of them. Mr. Tulley, no ways daunted by this first failure, recommenced his labours with that ardour for overcoming difficulties which seems to characterize his family; though it was confidently asserted at the time by some individuals, (whose opinions were justly entitled to the highest consideration from their distinguished rank in the scientific world,) that the object he had in view was an unattainable chimera, and who ridiculed the project accordingly. Indeed it must be confessed that the obstacles which presented themselves to the formation of these small achromatics (*at least when perfected with a sufficient and effectual aperture*) are so great that in all human probability they would have proved insuperable by the skill and powers of any other individual. However frivolous and contemptible they may appear to some, both in themselves and in their uses and applications, I shall venture to assert (with humble deference to wiser men) that the construction of them is one of the greatest achievements which has yet been performed in practical optics, and far surpasses in difficulty the making of object-glasses of long foci for telescopes, (which is known to be no easy matter.) I think it highly probable also that their theory will prove proportionally complicated and abstruse, and will exercise the genius of the mathematician, as much as the working of them by trial has done that of Mr. W. Tulley. In order duly to appreciate the difficulty of making these small object-glasses—suppose it was required to shorten the focus of an object-glass for a telescope of 4 inches of aperture, from 60 to only 9 inches, and to execute it as perfectly as before, would not this be a great undertaking? But suppose again that the scale of its dimensions was to be reduced from inches to tenths of an inch, so that it should have $\frac{4}{10}$ inch aperture, and $\frac{9}{10}$ focus, and still be worked as correctly as before! Whosoever may choose to try to make such a curiosity will find out to what a degree every difficulty is aggravated by working on so small a scale. It may be safely affirmed that an error of much less than the $\frac{1}{1000}$ of an inch in the radius of the curves, in the working of them even or true, or in the thickness of the glass, will be sufficient to ruin the goodness of the lens. Besides working the lenses as true as it is possible to imagine, the centering of them is an affair of the last importance, particularly with respect to the concave lens; and the thickness of the glass operates so strongly, that if the concave lens of a perfect object-glass is detached and rubbed for a few seconds on a piece of ground glass only, so that its rims shall be slightly abraded, and reduced in thickness by some immeasurable quantity, the composition will be spoiled, and the balance of the aberrations overturned. The adjustment of the lenses is another difficulty, and must be performed as truly as in telescopic object-glasses, or the *maximum* of distinctness will not be obtained. In the process of working, a great inconvenience arises from the tools having a perpetual tendency to alter their figure: which circumstance renders it impossible to work two object-glasses alike, so that a dozen might be ground out of the same glass, and in the same tools which had formed a perfect one, which yet would be all good for nothing; moreover it sometimes happens that an object-glass shall have neither chromatic nor spherical aberration, and yet be utterly incapable of showing an object tolerably, from a defect probably in the material, as sometimes also happens in telescopes. A due consideration of these untoward circumstances will serve to show that the task of making one of these small *aplanatics* is even more arduous than it would appear to be à priori. Mr. Tulley directed the whole of his spare time during one twelvemonth to the subject before he could in any degree succeed; and the repeated failures and disappointments which occurred were so very disheartening, that the project was nearly abandoned more than once; not as a physical impossibility in itself, but merely as impracticable in point of execution. After having tried them double, with two and afterwards with three convex surfaces on Mr. Herschel's plan, the concave being next the radiant, (which is its best position,) it was found that the depth of the curves would not admit of a sufficient aperture to exhibit the test-objects: this form was therefore given up. At length, however, triple lenses being tried, Mr. T.'s indomitable perseverance and constancy were rewarded by the production of one of 0.333 focus and 0.2 aperture; which, though not so perfect as some which have been since made, showed all the test-objects in perfection. This memorable object-glass (being, I believe, the first *effective one* that was ever made) was completed about the 1st of March, 1824, to the eternal honour of the name of Tulley, and is now in my possession. I would not wish to depreciate the merits of those opticians who have produced large object glasses and metals for telescopes; but I am disposed to think the difficulties of making these choice gems of art, on a very small scale, fully equals that of making them on a very large one. It is perfectly well known that the difficulties in the construction of any piece of exquisite machinery are wonderfully increased and aggravated, if the artist is compelled to work upon an exceedingly small scale. I present these considerations to my reader that Mr. T.'s praiseworthy labours may be justly appreciated, or at least not ridiculed by the unthinking.

Mr. Tulley, emboldened and inspirited by his success, determined to follow it up, and to leave nothing to be done by any man who should take up the subject after him; and as I had constantly represented to him the superiority of object-glasses of *short* focus, (provided they were equally perfect, on account of their greater power, and the natural magnitude of their image with those of longer foci,) he actually set about the task of making one of only 0.2 focus ! and moreover accomplished it in about two years after his first essay ; having met with such a series of ill-luck as would have distracted the brains of an ordinary mortal, in the course of his labours on it. This miracle of art is also in my possession : it bears as large an angle of aperture as those of .933 focus*, and, but that the glass is thicker than is desirable, (which causes it to exhibit round bodies somewhat oval towards the edges of its field of view, at least when charged with a low power,) leaves little to be wished for or desired in point of performance ; unless, perhaps, an increase in its acting angle of aperture with a certain class of objects.

It must be obvious, on the most superficial consideration, that the vast thickness of the material of which these small aplanatics are made, is a very serious evil; more especially as it is absolutely necessary to work on a very considerable substance in order to obtain a spherical figure in the curves with any sort of precision. My friend, Mr. J. Lister, was so much influenced by these considerations, that after Mr. Tulley had made the two object-glasses I have already mentioned, he caused him to construct one of longer focus, (of which I have given the dimensions in the plate, fig. 13,) justly conceiving that it might be brought to a much greater degree of perfection than the smaller ones, and so fully to rival their performance ; gaining more by the accuracy of its execution, and the thinness of its material, than it would lose by the smallness of the image it would afford (cæteris paribus) when compared with the others. The result of this experiment justified Mr. L.'s preconceived opinion ; for the performance fully equalled that of the 0.4 first made, and far surpassed my most sanguine expectations from an object-glass of so long a focus, though it fell greatly short of that of the 0.2, as might naturally have been expected. Mr. L. was of course highly gratified by the result of this trial, and immediately instigated Mr. T. to attempt another, which was to have a focus of only 0.3, and in its other dimensions to be a miniature of the 0.933. Mr. Tulley was a very long time in bringing this object-glass to perfection, the thinness of the material being a most severe drawback upon its progress;—but his usual constancy and unrivalled skill at length prevailed; and this 0.3, with an acting aperture of 0.15, may be considered as his present chef-d'œuvre. It is sufficient for me to say of its performance, that it decidedly surpasses that of my 0.2, which I am persuaded it would not do if the other was equally thin and perfect.

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* As before stated, its focus is 0.2 only, (as ascertained by the measurement of the power of a telescope, to which it was attached as an eye-glass,) yet it carries an aperture of 0.1, with perfect distinctness on opaque objects of the most difficult definition : whereas the utmost aperture those of 0.933 will bear, even when in the most perfect adjustment, does not exceed 0.4 upon the same objects; upon transparent bodies both will bear an extension of aperture to the entire diameter of the glass.

The name of Lister must descend to our miscrosophic posterity as a most indefatigable labourer in the vineyard, under whose auspices Mr. Tulley has succeeded in carrying the perfection of these small aplanatics to a much higher point than I am persuaded he would ever have done without the stimulus of this gentleman's ardent zeal and exertions in the good cause. This seems to be the golden age of microscopes,—the finishing stroke has been given to their exaltation among scientific instruments, by the formation of aplanatic lenses of adequate depth and aperture to become effectual in performance,—by the perfection of the Amician reflecting microscope,—and the formation of single lenses from the diamond. No improvements at all commensurate to these in magnitude and importance can, I predict, be ever made hereafter. Mr. L. has conferred other obligations on microscopic science, by directing Mr. T. to construct an erecting eye-piece for compound instruments, which renders them invaluable tools for dissection, and other operations on minute bodies, from their allowing abundant room for the management of instruments, &c., between the object-glass and stage, though a very high power should be employed on opaque bodies, such as we should vainly attempt to use with a single microscope even on transparent subjects. He has, moreover, laboured incessantly to perfect the stands and apparatus of microscopes; and has discovered a method of measuring the curves of the small achromatics with great precision, which may prove of high utility in verifying their theory. As, however, he will, I trust, give to the world the result of his labours in his own proper person, I do not feel myself at liberty to anticipate his proposed essay on those subjects.

The history of the conception, progress, and final completion of the new aplanatics in this country, is finished up to the present period, when I state, that Mr. Tulley is endeavouring to make one of only $\frac{1}{10}$ of an inch focus* ! Considering what he has done, it is not totally impossible he may one day succeed: however, I much doubt if the performance of it will surpass that of the 0.3 focus, unless the component lenses can be made equally thin in proportion to their focus, and as perfect in all other respects.

At Fig. XIII. is a true representation of the object-glass of .933 inch focus magnified twice: it will be observed, that the concave lens is so thin, that it is almost ground through in the middle: this is very essential to its perfection, but very difficult to accomplish. The surfaces are numbered according to the order in which the rays pass through them from the radiant point. The exterior convex is of crown glass; the interior one of Dutch plate. The radii of the curves and other dimensions, &c., are given as nearly as Mr. T. could measure them by gauging his tools; but it must not be supposed that they approach to the accuracy obtained in theoretical calculation, or that they are as truly given in proportion as the radii of object-glasses of long focus may be by similar measurements, for in this case they would require to be extended to many places of decimals; but every artist may rest assured, that he will never be able to execute these curves with accuracy even to the third place of decimals, and the measurements are, therefore, quite close enough for practical purposes.

The sidereal focus of the object-glass then is 0.933 inch.

The total diameter of the lenses 0.55 inch.

The clear aperture - 0.5

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* The figure of this object-glass is by no means bad even in its present condition, but it is so much over corrected in point of colour, that it is as chromatic as a common lens.

The perfect aperture	0.38	
Radius of 1st surface	0.825	crown.
2d -	0.525	convex.
3d -	0.5	flint.
4th -	0.5	concave.
5th -	0.575	Dutch plate.
6th -	0.575	convex.
Specific gravity of the crown glass	2.527	
Thickness of ditto - -	0.15	
Specific gravity of flint glass -	3.627	
Thickness of ditto - -	0.164	
Specific gravity of Dutch plate	2.519	
Thickness of ditto - -	0.175	

The curves and other dimensions of object-glasses of all other foci, may be obtained from these by simple proportion.

As these small aplanatics have been worked entirely by trial upon near microscopic objects, exactly as a telescopic object-glass is worked upon distant ones, very little can be said about their theory: experience has shown that the thinner the component lenses can be made, and the more closely curves are compacted together, the better. It will be remarked, that the figures which have been selected by Mr. Tulley approach to the double convex. The concave is, or is intended to be, quite isosceles; and so is the convex of Dutch plate. The reason of his giving these curves equal radii, at least only differing from each other by accidental errors in the process of working, is to allow of every possible permutation and combination of them, so that by chance, as it were, a happy result may be obtained, for there is no sort of certainty in the proceeding, because the figures of the tools change so eternally, that the artist never knows precisely what he is about, nor what the curve he is forming will turn out to be; on which account I suspect it will ever be impossible to work to any regular theory, if we had one. Doubtless, however, it is a fine subject to exercise the powers of the algebraist. These figures also evidently give the largest possible aperture, a point of the last moment, to enable them to distinguish certain objects, which they are thus enabled to do, though the whole of their calibre should not be perfect: where a reduced diameter, however aplanatic, would be ineffective. Whosoever may choose to divert himself with the theory of the object-glass of a microscope, must always remember to adapt his calculations to the *largest possible angle of aperture* which the necessary depth of the curves will admit of, for even with isosceles lenses the necessary aperture can scarcely be attained. A perfect microscopic object-glass is entirely useless when made to act upon parallel rays, like that of a telescope, the separate figures necessary to correct spherical aberration for each being as widely different from each other as an ellipse from a parabola. I forgot to state, that the ratio of the curves of Mr. T.'s master-piece (Mr. Lister's 0.3), are as nearly as possible the same as those of the 0.933* I have given; the superiority of the former residing entirely in the shortness of its focus, and the consequent natural magnitude of its image. It is here proper to observe, that the contraction of the apertures of the small aplanatics is best performed by a stop at their posterior surface, or that next the eye-glass, because they act upon diverging rays; if cut off at the anterior surface smaller apertures must of course be used to produce the same reduction of the size of the visual pencil for the same reason. It would be very adviseable to have a slide with holes of various diameters, with a spring catch, to pass before the posterior surface of the glass, to adapt the aperture to the particular object under consideration. It is well known that the vision of many transparent objects, not requiring much light, or a large angle of aperture is improved by placing a stop or cone of small diameter behind the stage. Now the virtual operation of this upon the intercepted light is to reduce the aperture of the object-glass, and consequent size of the pencil at the eye, not that it indeed obliterates it, but only renders as much inert as exceeds in size the image of the stop, for by moving the eye about, every part will still be brought to operate on the retina, in successive portions; but as to light, the effect is exactly similar to that obtained by cutting off the aperture of the object-glass: now it has always appeared to me, that whenever it is advisable to produce the effect of the cone, it will be much better to operate on the object-glass directly, by which means a great increase of distinctness is obtained. Few understand completely the nature of vision by intercepted light; but it will be very easy to verify what I have asserted, by examining the vision pencil of a microscope with a magnifier, with and without a cone behind the stage.

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* Though Mr. Tulley believes that the curves of this object-glass, together with its other dimensions, are true miniature copies of the 0.933; yet it bears a decidedly larger angle of aperture: the reason of this I conceive to be, that all lenses of short foci will, from the natural smallness of their apertures, and consequent rarity of their light, render aberration less sensible than longer ones of proportional aperture, equally perfect: an eternal argument for short foci. A common object-glass of $\frac{1}{10}$ inch focus, and $\frac{1}{20}$ inch of aperture, acts infinitely better than one of 1 inch focus and $\frac{1}{2}$ inch in diameter, as every optician knows.

Mr. T.'s object-glasses, by virtue of their natural formation, correct along with the achromatic eye-pieces, which were attached to them in the process of working, so that the totality of the instrument is freed from aberration, except the trifling tinge of green and purple which results from the secondary spectrum, and is left after the equal and opposite dispersion of the flint and crown glass, and cannot be remedied. Thus a great advantage is obtained over single lenses, the aberrations of which are totally uncorrected; their error from sphericity alone always amounting to a quantity considerably greater than their thickness, though of the best possible figure. The pencil of rays from the compound instrument must indeed be duller with any given illumination than the cylindrule from the single lens, being weakened by four extra refractions; but it must be recollected, that microscopes do not resemble telescopes in the nature of their light; because in the former we can illuminate artificially, and thus compensate for any loss of rays by transmission through several surfaces, and after all, frequently obtain more light than is necessary. It is on this principle, that two telescopes of the same aperture, but one acting by reflection and the other by refraction, would answer equally well for viewing such a body as the sun, though the ratio of their natural light, or penetrating power, should be as 5 to 8.

It will be found that single lenses do not possess any marvellous power of penetrating into the texture of objects, beyond what results from their being susceptible of larger apertures than the object-glasses of common compound microscopes, and in this their superiority consists. When the object-lenses of the compound instruments have been freed from their aberration, so that their caliber can be extended to the due point, they will be found to exhibit every thing to be seen by the very best single microscopes, power for power, with a smaller angle of aperture, because the marginal rays are rendered true and effective. If they do fail in any instance, it will be only when the depth of their curves precludes the possibility of giving them the requisite diameter, and consequent due breadth to the visual pencil of rays. I presume it will be admitted, that whatever microscope gives vision most free from aberration, must be, or at least ought to be, the best; and I am happy to be able to observe, that the aplanatic engiscopes of Mr. T.'s construction have been thought by the best judges to show objects more satisfactorily than single lenses of the most exquisite workmanship, though the former shows only a simulacre, or representation of the object, while the latter shows the original in nature itself, from which the picture was formed. Such is the effect of destroying aberration!

I must observe generally, that whatever I have said in my paper on the Amician microscope, applies much in the same manner to Mr. T.'s instruments, for the same reasons.

It will now be proper for me to describe the various uses to which the aplanatic lenses may be rendered subservient.

1st. *As single magnifiers.*—In this form their effect is not so striking as might perhaps be anticipated*. It is something like that which is obtained by converting a telescopic object-glass into a reading-glass, except that the figure of the latter, when used with its anterior or external surface next to the eye, is correct for the purpose designed, whereas that of the other cannot be so in either position; nevertheless, objects are shown by it without any coloured fringes, and with greater distinctness, with a larger angle of aperture, than by any other combination of glasses.

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* Diamond lenses I conceive to constitute the ultimatum of the perfection of single microscopes. My friend Mr. G. Francis has calculated the spherical aberration of a plano-convex diamond lens for parallel rays; it amounts to only 0.949, enunciated in terms of the thickness of the diamond, while that of glass is 1.166; but when it is considered that the magnifying power of a diamond lens is to that of a glass one, ground to the same radius, as 8 to 3, the said thickness comes to be a very small quantity, where the power and aperture of the glass and diamond are equalized. In fact, the longitudinal

2d. *As eye-pieces for astronomical telescopes.*—In this application they may probably be found useful in very delicate measurements, where it would be necessary to get rid of those coloured fringes which are produced by the positive combinations of glasses which must be employed with micrometers, and which it is well known cannot be rendered achromatic.

Mr. T.'s lenses can be made deep enough to charge a telescope with sufficient power, and may be so figured as to remove all distortion from the cobwebs of the micrometer; but the field of view cannot well be made to exceed 15 degrees.

3rd. *As secondary object-glasses in the erecting eye-pieces of*

aberration of a diamond magnifier, equal in power and aperture to a hemisphere of glass of any focus, is less than one-sixth of that of the latter substance, though the disproportion becomes less when smaller angles of aperture are employed. The chromatic aberration of the diamond is known to be about equal to that of a drop of water of the same radius, (which is a mere trifle.)

Mr. Andrew Pritchard, of 18, Pickett-street, Strand, made the first diamond lens at my instigation, (while under the tuition of Mr. C. Varley.) He has now just completed a beautiful double isosceles convex, extremely thin, of the finest water, and free from all flaws; its magnifying power is abundantly effective for practical purposes. Its polish is very high, and flashes on the sight with an almost metallic brilliancy, so that the most careless observer can scarcely fail at once to recognise the invulnerable substance of which it is composed, especially when compared with a glass lens. I have little doubt that a diamond lens might be made of $\frac{1}{200}$ of an inch focus: indeed, Mr. P. will undertake to form one of that power; for such is the enormous refraction of this imperishable and beautiful substance, that it would only require to be ground in a tool requisite to form a lens of glass of the $\frac{1}{75}$ of an inch focus.

Mr. Pritchard will be proud to exhibit this curiosity to amateurs of microscopic science, and more especially to those sages who, for certain excellent reasons of their own invention, choose to believe that diamonds cannot be fashioned into spherical curves.

spy-glasses.—It must be obvious that these being in their nature and principle true compound microscopes, must require their secondary image as perfect as possible; it should, therefore, be formed by an aplanatic lens: nevertheless the combinations in common use answer abundantly well, because they do not require an angle of aperture larger than that of the primary one, which is always very small: if, however, their focus was much shortened, so as to render the instrument dumpy, for the convenience of being held more easily in the hand, like a common perspective, (which would be the greatest improvement which can now be made upon them,) it will then be found that a regular secondary achromatic object-glass becomes indispensable, because the stop between the two bottom glasses in the ordinary construction would in such a case require to be opened out, till the aberrations of both kinds would become very strong—at least if the whole pencil of light from the primary object-glass was admitted through it; of which it is easy to give demonstration by adapting in this way a common erecting eye-piece to the object-glass of a perspective, having, perhaps, an aperture of an inch and a half to a focus of only five inches, taking care that none of it is cut off by the said stop, which it may be as effectually as by a cap. I know very well that an erecting eye-piece may be rendered achromatic by a combination of convex lenses alone; namely, by a reduplication of the Huygenian, or astronomical eye-piece, and that also to any required angle of aperture; but, unfortunately, nothing can be worse than this composition for giving distinctness, accordingly it is never used by opticians, even to work with a very small angle of aperture. Nothing but a concave of a certain figure and density has the power of destroying both kinds of aberration at once in convex lenses.

4th. *As magnifiers for the solar microscope.*—It must be evident, that whatever kind of object-glass will answer best for a compound microscope, must also be the most perfect for a solar one, both having to form images as truly as possible: one on the wall of an apartment, the other in the field-bar of the compound body, to be viewed there by an eye-glass: accordingly, the small aplanatics effect the same kind of improvement in solar microscopes as in compound ones, for the same reasons. In the solar instrument, however, the entirety of the image formed by the object-glass being spread out upon the wall in its extreme dimensions, as in the camera obscura, all the imperfections of the marginal rays are displayed. By an unlucky fatality, all those figures and combinations of lenses which will give central rays most perfect, give marginal ones the worst, and vice versâ. This observation holds good with regard to the aplanatics, which used as single solar magnifiers, give the central portion of the picture beautifully distinct and achromatic, but the circumferential part very confused: (which marginal portion is excluded, and cut off by the field-bar of the eye-piece of a compound body, and is not there visible.) If, however, an extravagant field of view is not demanded, there is nothing left to be wished for in point of brilliancy, distinctness, and achromatism. Care must be taken, however, in using these glasses, in such a solar microscope, that the illumination is directed truly through their axis, and likewise that they are placed considerably within the focus of the illuminator, otherwise the coloured portion of the sun's image, formed by the prismatic edges of the illuminating glass, will get through, and contaminate the picture, just as if the object-glasses themselves were chromatic.

I am of opinion, however, that the best way of using the aplanatics as solar microscopes is to attach them to the body of the compound instrument they were made for, though in this way a little colour will be seen, (chiefly the secondary spectrum.) In this manner an ample field of view and abundance of power are obtained in a legitimate way, without suffering the rays to diverge far: indeed, if the maximum of distinctness is required, it is best to cut them short at 8 or 10 inches' distance from the eye-piece: thus transparent and opaque bodies of all kinds (even very difficult test-objects) may be seen in all the sharpness and vividness of miniature pictures, in which nearly every thing is visible, which can be seen by looking through the eye-glass.*

We may always with the greatest confidence predict the quality of a solar microscope, from considering its optical part separately as a single or compound microscope; such as *it* is, such will be the image formed by it. If it is deficient in light, distinctness, achromatism, or field of view, &c., we may be quite sure that all these defects will be aggravated when we cause it to form an image for the solar instrument. We may as well expect to gather figs from brambles, as to get a fine picture from an uncorrected convex lens. The image of a common solar microscope may be considered a mere shadow, fit only to amuse women and children, more especially if we attempt to exhibit brilliant opaque objects, which either become undistinguishable with a limited aperture, or enveloped in a glorious mass of aberration, with an enlarged one. The utmost it can do is to give us the *shadow* of a flea, or a louse as big as a goose or a jackass—

("Monstrum horrendum, informe, ingens, cui *lumen* ademptum.")

as a transparent object.

The swinish vulgar will always be gratified by such spectacles, because they have no idea that a microscope of any kind is to do more than exhibit objects very much dilated in point of bulk: if any optician could contrive an instrument, which would at one swoop take in the whole of a horse, ass, or elephant, and exhibit it (no matter how) with a power of about 240, to this class of observers, I am sure it would delight them infinitely more than the effects of the most beautiful achromatic lenses, which Mr. Tulley, or M. Chevalier can make.

[*To be continued.*]

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* I have an unique solar apparatus of this description. Mr. Cuthbert, 22, Bishop's-walk, Lambeth, can manufacture it for the curious: it is an expensive instrument, but to a connoisseur "le jeu vaut bien les chandelles."

ART. VI. *Improvements in the Solution of Equations by Continued Fractions.* *By* W. G. HORNER.

[Continued and concluded from page 81.]

V.

1. IN § 47, *et seq.*, of *Résolution des Eqq. Num.*, Lagrange has investigated a formula for continuing the series of fractions that converge toward the value of any continued fraction, which either from the first, or after an interval, is periodic. He had previously shown that such a fraction is always equal to the root of a quadratic equation determinable from it.

The results of his investigation are given in § 50 and 60, but in a form so inconveniently complicated, as to raise a doubt if they have ever been adopted in practice. This may be ascribed to his having, whether designedly or not, everywhere avoided the consideration of the ambiguous character of quadratic roots, except where it was forced upon him by the intervention of the sign of the square root.

Now as this ambiguity may be readily shown to exist in the series itself, it appears most natural to keep it in view from the commencement; and the simplicity of the result obtained in the following pages, by adhering to such a principle, seems to justify the presumption.

2. Let the circulating portion of the fraction be $\left(\frac{1}{c_1 +} \frac{1}{c_2 +} \ldots \frac{1}{c_n +}\right)\infty$, the law, both of the numerators (u) and denominators (e) of the converging series, is

$$e_v = c_v \, e_{v-1} + e_{v-2} \quad . \; . \; . \; . \; . \; . \; (1)$$

This equation, transformed, becomes

$$e_{v-2} = - c_v \, e_{v-1} + e_v \quad . \; . \; . \; . \; . \; . \; (2)$$

By the first equation, the series may be carried *forward* to any extent, as is well known. The second indicates, that by *changing the signs of the denominators* in the continued fraction, the series may be extended *backwards* at pleasure; a fact generally overlooked.

This infinite extension in both directions is in accordance with the true idea of a circulating fraction; for, out of innumerable periods, that which we pitch upon as the first in our operations is made such solely by our choice, and is, in fact, preceded as well as followed by innumerable periods.

Imagine, for example, the set of denominators, 2, 1, 3, 4, to recur in infinitum. We know the first converging fraction to be $\frac{1}{2}$, and the second $\frac{1}{3}$. These being placed beneath their proper multipliers in any one period, we find, by eqq. 1 and 2, the double series of fractions below.

$$\begin{array}{cccccccccccccccc} \ldots 2, & 1, & 3, & 4, & 2, & 1, & 3, & 4, & 2, & 1, & 3, & 4, & 2, & 1, & 3, & 4, \ldots \end{array}$$

$$\ldots\ldots \frac{47}{-11}, \frac{-17}{4}, \frac{13}{-3}, \frac{-4}{1}, \frac{1}{0}, \frac{0}{1}, \frac{1}{2}, \frac{1}{3}, \frac{4}{11}, \frac{17}{47}, \ldots\ldots \quad (3)$$

If this series be read from the left toward the right, it proceeds by eq. 1; if in the contrary direction, by eq. 2; in one uniform tenour. In reasoning generally, therefore, it may be regarded as one unbroken series. The separate and relative values of its two natural divisions will appear as we proceed.

3. From the law of continuation, by given multipliers and addition, it is manifest, that every numerator in a converging series may be represented by a simple binomial function of any two consecutive numerators; and the same applies to the denominators. For example, we shall have

$$e_{v+1} = c_{v+1} e_v + e_{v-1}$$

$$e_{v+2} = (c_{v+1} c_{v+2} + 1) e_v + c_{v+2} e_{v-1}, \text{ \&c.}$$

$$\text{until} \quad e_{n+v} = Ae_v + Be_{v-1} \ldots\ldots\ldots\ldots (4)$$

$$\text{and so} \quad e_{n+v-1} = ae_v + \beta e_{v-1} \ldots\ldots\ldots\ldots (5)$$

Again, the periodical return of the multipliers, whose aggregates make up the co-efficients A, B, a, β, will show that whatever function any term is of any two consecutive terms, the same function will a term taken at any entire number of periods distant from the former, be of a pair of terms taken at the same distance, and in the same direction from the latter. So that we also have

$$e_{nz+v} = Ae_{nz-n+v} + Be_{nz-n+v-1} \dots\dots (6)$$

$$e_{nz+v-1} = ae_{nz-n+v} + \beta e_{nz-n+v-1} \dots\dots (7)$$

the co-efficients remaining constant while v remains so.

Now, in the case of

$$e_{2n+v} = Ae_{n+v} + Be_{n+v-1}$$

substitute equations (4), (5), and it becomes

$$e_{n+v} = (A^2+Ba)e_v + (AB+B\beta)e_{v-1}$$

from which eliminate e_{v-1} by means of eq. (4), and it gives

$$e_{2n+v} = (A+\beta)e_{n+v} + (Ba-A\beta)e_v \dots\dots (8)$$

and, by the premises, any multiple of n may be added to all the exponents in this equation, without infringing on its accuracy. It remains that we find the values of the arbitrary co-efficients.

4. We know that $\frac{u_{n+v}}{e_{n+v}} = \frac{1}{c_1} + \frac{1}{c_2} + \dots \frac{1}{c_n} + \frac{u_v}{e_v}$

$$= \frac{u_n e_v + u_{n-1} u_v}{e_n e_v + e_{n-1} u_v} \dots\dots\dots\dots\dots\dots (9)$$

whatever is the value of v. Hence we have four equations,

$$u_{n+v} = Au_v + Bu_{v-1} = u_n e_v + u_{n-1} u_v$$

$$e_{n+v} = Ae_v + Be_{v-1} = e_n e_v + e_{n-1} u_v$$

$$u_{n+v-1} = au_v + \beta u_{v-1} = u_n e_{v-1} + u_{n-1} u_{v-1}$$

$$e_{n+v-1} = ae_v + \beta e_{v-1} = e_n e_{v-1} + e_{n-1} u_{v-1}$$

From these, making, for brevity's sake

$$g = u_n e_v - e_n u_v, \qquad h = u_{n-1} e_v - e_{n-1} u_v$$

$$k = e_n u_{v-1} - u_n e_{v-1}, \qquad l = e_{n-1} u_{v-1} - u_{n-1} e_{v-}$$

and bearing in mind, that for every value of v, we have

$$u_{v-1} e_v - u_v e_{v-1} = (-1)^v \dots\dots\dots (10)$$

we shall at last obtain

$$(-1)^v B = ge_v + hu_v, \qquad (-1)^v A = ke_v + lu_v$$

$$(-1)^v \beta = ge_{v-1} + hu_{v-1}, \qquad (-1)^v a = ke_{v-1} + lu_{v-1}$$

which produce

$$\left.\begin{array}{l} A + \beta = e_n + u_{n-1} \\ Ba - A\beta = (gl-hk)(-1)^v = -(-1)^n \end{array}\right\} \dots\dots (11)$$

These values, being independent of the variable v, prove that formula (8) is universal, or that

$$\left.\begin{array}{l} u_{nz+v} = (e_n + u_{n-1})\, u_{nz-n+v} - (-1)^n u_{nz-2n+v} \\ e_{nz+v} = (e_n + u_{n-1})\, e_{nz-n+v} - (-1)^n e_{nz-2n+v} \end{array}\right\} \dots\dots (12)$$

for every value of v and z. This is a remarkable property of pure circulating fractions.

5. From this it follows, by the general Theory of Equations, that if R, r be the roots of the equation

$$y^2 - (e_n + u_{n-1})y + (-1)_n = 0 \; \ldots\ldots \; (13)$$

we shall have universally

$$\frac{u_{nz+v}}{e_{nz+v}} = \frac{pR^z + qr^z}{p_1R^z + q_1r^z} \; \ldots\ldots\ldots\ldots \; (14)$$

the value of the constants remaining to be determined.

6. Put $S =$ the value of the fraction, if infinitely extended. Then is

$$S = \frac{1}{c_1} + \frac{1}{c_2} + \ldots \frac{1}{c_n} + S = \frac{u_n + u_{n-1}S}{e_n + e_{n-1}S} \; \ldots\ldots \; (15)$$

whence $e_{n-1}S^2 + (e_n - u_{n-1})S - u_n = 0 \; \ldots\ldots\ldots\ldots \; (16)$

an equation whose roots are equal to $\frac{y - e_n}{e_{n-1}}$.

Of the continued fractional value of the principal root, equation 15 affords us a distinct notion; and the same equation by reversion gives

$$S = -\frac{u_n - e_nS}{u_{n-1} - e_{n-1}S},$$

which, by Art. 17 of my former papers, gives

$$S = -c_n + \frac{1}{-c_{n-1}} + \ldots\ldots \frac{1}{-c_1} + \frac{1}{S} \; \ldots\ldots \; (17)$$

This, if S retains its originally assumed value, in eq. 15, is an equation of mere identity, all the fractions mutually destroying each other; but, taken as a new equation, it expresses the value of the other root s, and shows that this is equal to the ultimate fraction whose law is equation (2), or to the given series when traced *backwards* to infinity, (Art. 2.)

7. Hence, to determine the new constants, we have four equations; viz., when $z = 0$,

$$p + q = u_v, \qquad p_1 + q_1 = e_v$$

and when $z = \pm \infty$,

$$\frac{p}{p_1} = S, \qquad \frac{q}{q_1} = s.$$

The solution of these equations will produce

$$p_1 = \frac{u_v - se_v}{S - s}, \qquad q_1 = \frac{Se_v - u_v}{S - s}$$

$$p = Sp_1 = \frac{Su_v e_{n-1} - u_n e_v}{S - s} \quad \text{since } Ss = \frac{u_n}{e_{n-1}};$$

$$q = sq_1 = \frac{u_n e_v - su_v e_{n-1}}{S - s}, \text{ for the same reason.}$$

Wherefore, restoring the values of S, s, we now have

$$\frac{u_{nz+v}}{e_{nz+v}} = \frac{[(R - e_n)u_v - u_n e_v]R^z + [u_n e_v - (r - e_n)u_v]r^z}{[e_{n-1}u_v - (r - e_n)e_v]R^z + [(R - e_n)e_v - e_{n-1}u_v]r^z} \cdot \cdot \cdot (18)$$

for the general term of the series of fractions converging to the value of a continued fraction which is entirely periodical.

8. Hence we deduce the more general theorem.

For, if $a_1 + \frac{1}{a_2 +} \ldots \frac{1}{a_m +} \left(\frac{1}{c_1 +} \frac{1}{c_2} \ldots \frac{1}{c_n +}\right)^\infty$ is equal to the root of the equation $Ax^2 + 2Bx = C$, and $\frac{N_1}{D_1}, \frac{N_2}{D_2}$, &c., are the fractions which converge toward the value of the entire root, as $\frac{u_1}{e_1}, \frac{u_2}{e_2}$, &c., are those which converge toward the circulating portion; the former may be derived from the latter by the formula

$$\frac{N_{v+m}}{D_{v+m}} = \left(a_1 + \frac{1}{a_2 +} \ldots \frac{1}{a_m +} \frac{u_v}{e_v} =\right) \frac{N_m e_v + N_{m-1}u_v}{D\ e_v + D_{m-1}u_v} \ldots (19)$$

And on applying this transformation to eq. (14), introducing $nz + m$ for nz, it becomes evident that we may assume universally

$$\frac{N_{nz+v+m}}{D_{nz+v+m}} = \frac{PR^z + Qr^z}{P_1R^z + Q_1r^z} \cdot \cdot \cdot \cdot \cdot \cdot \cdot (20)$$

Where, making, as before, $z = 0$ and $\pm \infty$ severally, we have $P + Q = N_{v+m}$, $P_1 + Q_1 = D_{v+m}$, and $\frac{P}{P_1}, \frac{Q}{Q} =$ the two values of x, viz., $\frac{-B \pm I}{A}$ or $\frac{C}{B \pm I}$; making $I =$ the irrational part $\sqrt{B^2 + AC}$ of the root. The four arbitrary constants, being determined from these four equations, conduct to the general theorem,

$$\frac{N_{nz+v+m}}{D_{nz+v+m}} = \frac{(CD_{v+m} - [B-I]N_{v+m})R^z - (CD_{v+m} - [B+I]N_{v+m})r^z}{(AN_{v+m} + [B+I]D_{v+m})R^z - (AN_{v+m} + [B-I]D_{v+m})r^z} \quad \dots\dots\dots \quad (21)$$

For the more convenient use of this theorem, it may be well to recollect, that since I is pretty obviously invariable throughout the transformations by which the continued fraction is evolved, and consequently the same in the value of S (eq. 16), as in that of x here, it clearly follows, from a comparison of equations 13 and 16, that if M be put $=$ the greatest common measure of e_{n-1}, $e_n - u_{n-1}$, and u_n, the irrational part of R and r will be MI, so that we have $R, r = \frac{1}{2}(e_n + u_{n-1}) \pm MI$, without solving a second equation.

In the case of the pure square root, or $Ax^2 = C$, since $B=0$ and $m = 1$, we have $\frac{N_{v+1}}{D_{v+1}} = \frac{N_1 e_v + u_v}{e_v}$, and the general formula 21 becomes

$$\frac{N_{nz+v+1}}{D_{nz+v+1}} = \sqrt{\frac{C}{A}} \cdot \frac{TR^z + tr^z}{TR^z - tr^z} \quad \dots\dots \quad (22)$$

where T, t, are $= D_{v+1}\sqrt{C} \pm N_{v+1}\sqrt{A}$.

Here it may not be improper to recapitulate, that m represents the number of terms in the finite portion of the fraction, n the number of terms which circulate, and that though v is optional, it is now considered as restricted to the values 0, 1, 2 ... $n-1$ only.

9. To exemplify the whole, let us take the equation $75x^2 - 244x + 198 = 0$, whose principal root is

$$\frac{122+\sqrt{34}}{75} = 1 + \frac{1}{1+}\,\frac{1}{2+}\,\frac{1}{2+}\left(\frac{1}{1+}\,\frac{1}{1+}\,\frac{1}{1+}\,\frac{1}{1+}\,\frac{1}{3+}\,\frac{1}{3+}\right)^{\infty}$$

The finite portion gives the *improper* fractions,

$$\begin{array}{cccccc} 1 & 1 & 2 & 2 & & \\ \frac{1}{0}, & \frac{1}{1}, & \frac{2}{1}, & \frac{5}{3}, & \frac{12}{7} = \frac{N_4}{D_4} & \therefore \frac{N_{v+4}}{D_{v+4}} = \frac{12e_v + 5u_v}{7e^v + 3u_v} \end{array}$$

and the circulating portion, the *proper* fractions,

$$\begin{array}{cccccccc} 1 & 1 & 1 & 1 & 3 & 3 & & \\ \frac{0}{1}, & \frac{1}{1}, & \frac{1}{2}, & \frac{2}{3}, & \frac{3}{5}, & \frac{11}{18}, & \frac{36}{59} = \frac{u_6}{e_6} & \therefore v < 6 \end{array}$$

The greatest common measure of 18, (59−11), and 36 is 6, wherefore $R,\ r = \frac{1}{2}(59+11) \pm 6\sqrt{34} = 35 \pm 6\sqrt{34}$; and by eq. 17, reduced by the same common measure,

$$\frac{u_{6x+v}}{e_{6x+v}} = \frac{\{(\sqrt{34}-4)u_v - 6e_v\}R^x + \{(\sqrt{34}+4)u_v + 6e_v\}r^x}{\{(\sqrt{34}-4)e_v + 3u_v\}R^x + \{(\sqrt{34}-4)e_v - 3u_v\}r^x}$$

is the terminus generalis of the periodical portion. Whence, by equation 18,

$$\frac{N_{6x+v+4}}{D_{6x+v+4}} = \frac{\{78e_v + 16u_v + (12e_v + 5u_v)\sqrt{34}\}R^x - \{78e_v + 16u_v - (12e_v + 5u_v)\sqrt{34}\}r^x}{\{46e_v + 9u_v + (7e_v + 3u_v)\sqrt{34}\}R^x - \{46e_v + 9u_v - (7e_v + 3u_v)\sqrt{34}\}r^x}$$

is the terminus generalis of the fractions converging to x; which might also have been deduced, independently of the periodical aggregate, from the result of eq. 21, viz.,

$$\frac{\{-198D_{v+4} + (122+\sqrt{34})N_{v+4}\}R^x - \{-198D_{v+4} + (122-\sqrt{34})N_{v+4}\}r^x}{\{75N_{v+4} - (122-\sqrt{34})D_{v+4}\}R^x - \{75N_{v+4} - (122+\sqrt{34})D_{v+4}\}r^x}.$$

10. In the *Annals of Philosophy* for April last, I have insisted upon the absolute distinction which exists between periodical formulæ of the first and second, and the higher orders. The present subject furnishes a curious illustration of this diversity, in the fact that formula 18, which, in its general interpretation, involves two variables, can, in the instances of the first and second powers, and of these only, as it would appear, with the exception of some partial examples, be reduced to a single variable.

With regard to the first power, or $n = 1$, this is evident; the only value of v being then 0. Since $\frac{u_0}{e_0} = \frac{0}{1}$ by formula 3, and $\therefore R + r = e_1 + u_0 = e_1 = c_1$ by formula 13, these substitutions reduce formula 18 to

$$\frac{u_x}{e_x} = \frac{R^x - r^x}{R^{x+1} - r^{x+1}} \quad \ldots\ldots\ldots \quad (23)$$

which is therefore the aggregate of z terms of the fraction $\frac{1}{c_1} + \frac{1}{c_1} + \&c.$; R, r being the roots of

$$y^2 - c_1 y - 1 = 0.$$

Again, since $\frac{u_{-1}}{e_{-1}} = \frac{1}{0}$ and $\frac{u_0}{e_0} = \frac{0}{1}$, we readily find, from formula 18, aided by 13, the two equations

$$\frac{u_{nz-1}}{e_{nz-1}} = \frac{[Ru_{n-1} - (-1)_n]\,R^{z-1} - [ru_{n-1} - (-1)^n]\,r^{z-1}}{e_{n-1}\,(R^z - r^z)}$$

$$\frac{u_{nz}}{e_{nz}} = \frac{u_n\,(R^z - r^z)}{(R - u_{n-1})\,R^z - (r - u_{n-1})\,r^z}$$

which, when $n = 2$, become

$$\frac{u_{2z-1}}{e^{2z}_{\ -1}} = \frac{(R-1)\,R^{z-1} - (r-1)\,r^{z-1}}{c_1\,(R^z - r^z)}, \quad \frac{u_{2z}}{e_{2z}} = \frac{c_2\,(R^z - r^z)}{(R-1)\,R^z - (r-1)\,r^z}$$

But, because $y - 1 = \sqrt{c_1 c_2 y}$ in the case when $n = 2$, this substitution being made for $y = R$ and r in these formulæ, reduces them, when z is introduced in the place of $2z - 1$ in the former, and of $2z$ in the latter, to the same value, viz.

$$\frac{u_z}{e_z} = \sqrt{\frac{c_2}{c_1}} \cdot \frac{R^{\frac{z}{2}} - r^{\frac{z}{2}}}{R^{\frac{z+1}{2}} - r^{\frac{z+1}{2}}} \quad \ldots\ldots \quad (24)$$

which is therefore universally the aggregate of z terms of $\left(\frac{1}{c_1} + \frac{1}{c_2} +\right)^{\infty}$; R, r being the roots of

$$y^2 - (c_1 c_2 + 2)y + 1 = 0.$$

Mr. Herschell, in *Examples of the Calculus of Differences*, Sect. 12, Art. 2, 3, has solved these two cases, and his results agree with our formulæ 23 and 24, but are considerably less simple.

Erratum.—In page 76, line 5 from the bottom, *dele* $[d]$; and insert $[d_1]$ on the right-hand below line 7 from the bottom.

Art. VII.—*Observations on the Geological History and Formation of Coal.* *By* J. Mac Culloch, M.D., F.R.S.

[Communicated by the Author.]

As there are few subjects in the range of geological investigation more important than the natural history of coal, so there is none which has experienced a greater share of attention. Yet we are chiefly indebted to practical miners for our knowledge, the inquiries of philosophical geologists having added little to the facts which they have received from that source. It cannot be said that there is a want of materials towards a history of this substance as it occurs in Britain; on the contrary, these have been displayed with all the minute refinement which so commonly attends subjects of great practical interest. Thus the history of coal, in England at least, seems at length understood; and if geologists have not reduced the whole of its appearances under any general rules, it may, perhaps, be attributed to some obscurity in the statements of practical men, arising from want of generalization, and partly to those ancient theories which have so long impeded the progress of geology. But though a correct notion may possibly be formed of the geological relations of coal in the British dominions, the information which we possess, relating to its disposition in other parts of the world, is far too meagre to permit a confident view of its entire connexions.

The coal of the more recent secondary, of the tertiary strata, and of the alluvial soils, as it has been called, has been described in a former paper in this Journal, under Lignite; its geological connexions differing from those of true coal, even more than its mineral nature. A portion of the coal found under basalt is also included under the same head. The remainder of that which has been improperly created into a division, by the name of basaltic coal, will naturally come under review as part of the history of the ordinary deposits. It does not form a distinct geological variety, since the phenomena by which it is characterized are no other than those which are seen in all the secondary strata that are disturbed by the intrusion of trap-rocks. Thus it only remains to consider those extensive and important deposits belonging to the secondary strata, which include almost all that is interesting in the history of coal. With what propriety or advantage this has been called the *independent coal formation*, must be left to the decision of those who understand the meaning and purpose of this exclusive term.

Yet as no opportunity has been afforded, in the former paper, for considering that coal which occurs among the primary strata, I must here give it a place; that in this and the former essay, the whole question relating to this mineral may be seen in one connected view.

The connexions of primary coal present very little interest or instruction, however interesting the mere existence of this mineral among primary rocks may be esteemed. Where it has been found, it has occurred in gneiss, in micaceous schist, in primary limestone, and in a conglomerate rock said to belong to the primary class. Thus it has been discovered in various parts of the continent of Europe, as in France, Norway, and Germany; but as yet no example has been observed in the British dominions. In all these cases, it forms very limited masses, being, in some instances, in detached lumps; in others, in forms said to resemble those of veins rather than strata. I need not say that a thin and non-persistent stratum is easily called a vein.

The origin of this coal remains a matter of doubt among geologists. It has been supposed, and with great appearance of reason, that the present mineral carbonaceous substances have once been vegetables, and that the hydro-carbonaceous compounds or bitumens equally owe their origin to the organic substances of former ages. When these occur among the secondary strata, they commonly carry their proofs of such an origin with them, as will be more fully shown hereafter. It may not be so easy to admit of this opinion in the case of primary coal; yet it does not imply any impossibility. I have shown, in speaking, on a former occasion here, of primary limestone, that organic animal remains do exist among the primary strata; and as I need not repeat the reasonings then given, this hint will suffice for the possible explanation of the origin of primary coal. That it should occur in limestone and in a conglomerate, are analogies to its position in the secondary rocks, of which the value is here obvious. Admitting even the hypothetical supposition of the absence of dry land in that original state of the globe which produced the most ancient marine animals, the growth of marine vegetables would be sufficient to explain the formation of carbonaceous matter; as these, even in the present state of things, are known to form deposites of peat. That these vegetables actually occur, and in the older schists, is now well known.

Primary coal thus offers an analogy to the lignites which occur in the upper secondary strata, rather than to the proper coal series; nor, if I have succeeded in showing that vegetables might have existed in some ancient states of the globe, is there any difficulty in accounting for its occurrence. We can conceive that organic animal remains might have been utterly destroyed by the revolutions which these have undergone; but the indestructible nature of charcoal, or the associated substances, when protected from air, explains, without difficulty, how vegetables, once entangled among the strata, should have been preserved, so as to have produced the coal of the primary rocks.

But as long as the true nature of elementary carbon remains unknown, this question must also remain obscure, or subject, at least, to dispute. The origin of the diamond is here the first difficulty which presents itself. If it be a true mineral production from elementary carbon, primary coal may also have the same origin. The existence of plumbago offers a parallel difficulty; and it occurs, as is well known, like coal, among the primary strata; in gneiss, and in argillaceous schist. If plumbago is of mineral origin, primary coal, or anthracite, which differs so slightly from it in its essential nature, may have been derived from the same sources. Yet this is far from certain. The coal of secondary origin, containing vegetable remains, is converted into plumbago by the influence of trap; and in my own experiments, detailed in the Geological Transactions, a substance scarcely differing from it was produced from wood. With equal reason, then, might plumbago, among the primary strata, be a modified coal, even originating in vegetables, as that each of them should owe an independent origin to elementary mineral carbon. But I will not attempt to illustrate further, by hypothetical speculations, a difficulty of this nature.

Yet I must here describe a fact of recent occurrence, which will probably appear to be of great importance in the general history of all coal, as well as of the merely primary; regretting that my information is nearly limited to specimens. This is the existence of extensive beds, as it is said, of anthracite, or primary coal, in Pennsylvania. These are found in what appears, in the specimens, to be unquestionable quartz-rock; and such it is said to be by those who have examined the ground. Should this fact be correctly stated, it will almost prove the views here held out, from the analogy of quartz-rock and the secondary sandstones; while it will thus also indicate another resemblance between these, and further tend to prove that an extensive vegetable creation occupied some parts of the ancient globe; connecting, at the same time, the more doubtful coal of gneiss and the old rocks with that of the secondary strata. But whatever views may be opened by this discovery, it is unsafe to speculate on them till the facts have been more correctly described.

I must yet notice here some deposites of coal which are of more recent origin than the primary, but which do not belong to the proper coal series. They are occasionally found in the old red sandstone, as appears from some foreign observations; though a doubt may rest on some of these, in consequence of the confusion often made between that stratum and the red marl. The coal of Arran, which appears to belong to this, is of such thickness as to have been wrought; and has a character approaching to that of anthracite, so as to resemble the more purely primary coal. Thus also coal has been found in the mountain limestone, but in insignificant quantity. These admit of the same remarks as primary coal; but they perhaps serve to confirm the origin assigned to that, by establishing a perfect gradation of this substance through all the strata; while the presence of organic remains in these rocks indicates the analogous origin of the carbonaceous matter.

I must now proceed to the history of the coal series which has been called independent, or to the more proper object of this paper. This is the deposite which forms the great repository of that mineral in England, though not the exclusive one; and of which the leading character, as it is known to us, and best known, is to occupy a geological position superior to the old red sandstone, and inferior to the new one, or to the red marl. But I must premise, that however I may have failed in truly classing under this head some of the instances here quoted, partly from want of accurate descriptions, and partly from unwillingness to doubt, without sufficient reasons for doubting, I intend under this title to include only the noted deposite just mentioned; to which also the general remarks and descriptions of most observers apply. This is the proper coal series; and it is here intended to exclude all coal-beds which follow the red marl, though I have not attempted to do that in specific instances. The term *independent*, already objected to, can produce only confusion; since it may be equally applied, as it has actually been, to the superior combustible deposites, equally independent, which, from their geological differences and mineral general characters, are best ranked with the lignites. To commence with the history of the series itself.

As the beds of coal are found accompanying and alternating with stratified rocks, so they are also disposed in strata parallel to these. Such strata are in every respect analogous, in their forms, dispositions, and accidents, to those of the rocks with which they occur. In position, they are horizontal, or inclined at various angles, often highly elevated, as is the whole series; circumstances which often lead to their discovery, or facilitate their working; but which also, on the other hand, frequently carry them out of the reach of ordinary mining operations. To name instances of these various positions would be superfluous; as almost every coal-field in Britain presents examples of them. If, in the descriptions of coal strata, practical miners have dwelt with undue stress on these and the other accidents to which they are subject, it must be attributed to the nature of their peculiar pursuit, the great importance of the object, and to their limited geological knowledge and imperfect acquaintance with the disposition of the other statified substances in nature. The philosophical geologist views in them examples only of general laws, which have influenced the forms and dispositions of all the strata of the earth.

As the coal strata, or the whole series, may be simply inclined, so they are frequently curved; a fact equally analogous to those which occur among other deposites, but which are often very valuable in a practical view, as restoring to the surface those beds which, had they been prolonged in the plane of their first inclination, would have plunged beneath the reach of mining operations. In many instances, such curved deposites thus form single basins, analogous to those which occur in various other cases; a general concavity being found in the central parts, and the edges appearing at the surface in various places around the circumference.

It is not unusual, also, for beds of coal to present continuous undulations, more or less marked, and accompanying, as might be expected, the general undulation of the associated rocks. From this cause, or from mere disturbance, they are sometimes found in convergent or divergent strata; offering circumstances of great practical moment wherever they occur. Of similar importance are the ordinary fractures, to which the beds, in common with the rest of the strata, are liable. As is usual among other strata, such fractures are attended by shifts; these being either simple, or accompanied by veins of other materials. In practice, they produce the sudden elevation or depression of the stratum in which the work is carried on; the consequence, in either case, being generally a source of much labour and expense.

The thickness of coal strata varies, even from less than an inch to ten or twelve feet; but it rarely exceeds two or three feet, and it is more often much less. It is frequently also variable in the same bed; and thus particular strata become extenuated till they disappear. Sometimes these beds have a schistose structure; in other cases they are massive; and in both they are often divided, like the argillaceous schists, by joints more or less parallel, at angles to the planes of their stratification. A few rare instances are on record, where the beds of coal have assumed a more regular prismatic or columnar structure; but this has been observed only in the vicinity of trap, and it is generally accompanied by considerable changes in the nature of the mineral.

There is, however, a rare modification of form in coal which I must not pass over; though, without a drawing, no precise idea of it could be conveyed to those who have not seen it. I know not that it has yet occurred anywhere but in the Glamorganshire coal-field, where it is not uncommon. It has been called a crystallization; a term, however, which is misapplied, since the form is in no respect geometrical. To say that the figure represented by a surface of coal of this nature resembles what, in pattern-drawing, is called snail-creep, and that the body of the mineral which forms this figure consists of a mass of vertical fibres, about an inch high, the intervals being generally about half an inch asunder, and, in the natural state, filled with a crumbling coal that falls out so as to leave vacancies, is the only manner in which I can convey a notion of this appearance by words. No explanation of this can yet be offered; but there is apparently no organic form from which we can suppose it derived.

It sometimes happens that only single beds of coal occur in one place; but, more frequently, they are repeated at intervals, being separated by those various rocky strata by which they are at the same time enclosed. Such repetitions of strata have been known to amount to thirty at least, as in Derbyshire; but they are seldom so numerous. Alternations extending from three or four to twelve, are more frequent; and in these collections of strata, the beds are not only unequal in thickness, but very different in quality; so that, either from their insignificant quantity or their bad quality, it rarely happens that more than two or three, even in a considerable series, are worth working. It must have been understood from former observations, that the coal series is not everywhere found among the secondary strata, however steady its place may be where it does occur, but that it forms distinct portions, often widely separated from each other. These portions are known, technically, by the name of coal-fields; and they vary in their characters in different places, not only in their extent and in their depth, but in the order of succession of the integrant rocky strata, in the numbers and relative proportions of these, and in the numbers, thickness, succession, and qualities, of the beds of coal. They must, therefore, be considered as independent deposites, varying as other local collections of strata are known to do, and from analogous causes; namely, from having been deposited originally, by independent actions, in independent cavities.

The rocky or other strata which accompany the beds of coal, and which contribute to form what is here called the series, consist of sandstones, shales, limestones, and clays. The sandstones are the most abundant, and the limestones occur chiefly, in some instances solely, in the inferior parts of the deposite; where a species of transition seems also often to take place between the proper coal series and the mountain limestone beneath it. The characters of the sandstone vary; being in some places a conglomerate, but more frequently fine, when it is sometimes compact, pure, and white, at others micaceous, or argillaceous, or ferruginous, and tender; occasionally also containing pyrites, and often blackened by carbonaceous matter, or else including distinct fragments of charcoal. Hence these strata also present various colours. With respect to the beds themselves, they are either massive and thick, or divided into thinner laminæ by intermediate clay or shale, so as to descend even to the tenuity of roofing-slate. At the lower part of the general series, this sandstone is often a conglomerate, intermixed with shales, to which the name of millstone-grit has been given in England; and, in this, limestones also occur. These resemble the inferior, or mountain limestone, though commonly more bituminous, and of a blacker colour.

The shales present all the variety of aspect and hardness which belong to this substance in the secondary series; passing at length into clays equally various, and sometimes containing bitumen, as well as carbonaceous matter and vegetable fragments.

In the clays are occasionally found imbedded nodules of argillaceous ironstone, often in conspicuous quantities; and these form the principal supply of ore for the iron-founderies. Galena and blende have sometimes occurred in very minute portions with the iron-stone; and pyrites is found in the coal itself, as well as in the accompanying strata, being the not infrequent cause of spontaneous combustion. When that substance which has been called porcelain-jasper exists, it must be considered as a casual and artificial mineral, produced by this cause. The most singular, as the rarest mineral, observed in this series, is an inflammable substance, imbedded in very small quantities in the ironstones; bearing, in its texture and general characters, a very strong resemblance to spermaceti, and now known by the name of Hatchetine.

With respect to the general position of the coal series as to the accompanying strata, above and below, though I have already said that it is situated between the older and newer red sandstones, it is impossible to give an universal description of its geological connnexions. The exact relations, distribution, and number of the strata, throughout even Europe, are far from being satisfactorily ascertained, as I have formerly shown; and as to the world at large, our ignorance is far greater. The difficulty has been much increased by confounding the upper deposites of coal, which belong to the lignites, with those of the proper coal series; the mineral alone, as is too common with geologists in many other cases where rocks are concerned, having formed the chief object of attention, to the exclusion of the geological characters and positions.

I must, therefore, describe that position as it occurs in Britain, where the subject has alone been accurately studied. Here then, supposing the series complete, the old red sandstone and the mountain limestone are followed by the coal series, to which succeeds the magnesian limestone and the red marl, and, subsequently, such strata of those above this as may be present in that particular tract.

Now it is essential to remark, that the old red sand-stone, the mountain limestone, and the coal series, are all disturbed; that they are elevated, undulated, and fractured, in various ways, as is far too notorious to detail. And it must also be recollected, as was equally remarked before, that a new order commences with the magnesian limestone and the red marl; or that they are placed on the coal series and the inferior strata, in an unconformable position, while the lower substance also presents that conglomerate structure which, everywhere throughout nature, accompanies a new order in rocks.

Hence the three deposites, or the coal series, and those which may be distinguished as forming the first division of the secondary rocks, have been united, as forming one class, and as if they had undergone but one disturbance which was common to the whole. But it would be easy to prove, by what I must here reserve for another paper, from the length of the discussions which it involves, that the coal series is really distinct, in time and production, from the inferior strata; and hence it cannot be always truly conformable to them, although the last general disturbance is common to both. This is a consequence which results necessarily from the very nature of the series and its formation; and if geologists have not always, practically, discovered the implicated relation between the coal series and these strata, it is because this previous view of that necessity has not been taken. The examination is not easy; and where great disturbances occur under such circumstances, it is natural to be content with that which the previous opinions seem to point out as the real state of things. It is to be expected that future observations, proceeding on this hint, will confirm the facts thus stated as necessarily existing.

With respect to other and remote countries, it is difficult to anticipate what the exact position of this series, if it really exists in other parts of the world, will be. That, at this particular period of the globe, it should have been produced in Britain only, or here and in a small portion of Europe, is not probable; as it would form an exception of such magnitude and importance, and as we have no reason, from what occurs regarding the other strata, to expect one so singular. We may, however, extend the general analogy, and expect to find the same series, if it should occur widely, following the lowest red sandstone, or at least inferior to the newer one, or to the saliferous strata. But, ignorant as we are of the earth in general, and uncertain whether its greater revolutions have been all simultaneous and general, it might be dangerous to make such a rule exclusive. Thus we must be content to wait for further information; and we must first, at least, wait till geologists have learned to distinguish more accurately among the secondary strata, and to give their true places to all those deposites of coal which occur above the saliferous sandstone, and which I have here separated from the true coal series. I must only remark, that since the inferior strata may be absent in particular places, as we have seen that they really are, and since even the superior ones may be similarly wanting where coal occurs, as is also the fact, this series may still be essentially corresponding, in other countries, with that of Britain, even where the same exact order does not take place. It must remain for geologists to extend their examination far more accurately and widely, before we shall be enabled to give a satisfactory account of the subject thus imperfectly sketched.

In an economical view, founded on this view of the coal series, it is now proper to observe, that it must be fruitless to search for coal below the old red sandstone, and, generally speaking, beneath the limestone immediately above that; as to mine after primary coal would be a wild project. It is almost equally useless to seek for it in those strata. It may also be unadvisable to attempt it, even in countries displaying the upper secondary strata at the surface, where indications at that surface do not exist; as it can rarely be known what the super-incumbent depth is, or whether, even if that were penetrated, the coal series, which is evidently a partial deposite, be present. The very series itself may be present, without containing beds of useful coal. Yet we cannot decide on its absence; and hence, wherever the red marl is found, it is always possible that coal may lie beneath it: and that it will thus be discovered hereafter, much more extensively than it has yet been, is far from improbable. Thus it may exist beneath the sands of Asia and Africa. And hence we derive the necessity of extreme caution in distinguishing between the older and the later sandstones. The rules for discovering coal in any country where it is known to exist, follow necessarily from this view of its connexions. It is superfluous, therefore, here, to record the technicalities of miners on this subject; as they are thus reduced to a simple principle. The same views will operate as a caution against the frauds too often practised by the interested; and thus further demonstrate the value of geological knowledge as a branch of general education, if any thing could convince the higher public of the utility of knowledge in statistics, public economy, and science.

It will be proper now to enumerate a few examples of the variations which occur in this series as it is best known to us. The coal of Gloucestershire succeeds to a sandstone, which follows the mountain limestone and red sandstone; and, after a certain number of alternations with the strata above-mentioned, it is followed by a coarse grit of great dimensions, and by a similar coal series succeeded by shale and coarse grit. Over this are placed, in unconformable order, the red marl formation, the lias, and the lower beds of the oolithe limestone.

In Derbyshire, there are enumerated thirty beds of coal, varying from six inches to eleven feet; the total thickness of coal being twenty-six yards. In this series, which extends downwards from the magnesian to the mountain limestone, there are twenty sand-stone beds, besides alternations of shale and clay of various kinds. In a section of the Durham and Northumberland coal-field, given by Mr. Forster, there are twenty-four beds of coal, alternating with as many of sandstone and shale, or of shale alone. These are succeeded, downwards, by a few more beds of coal, alternating with the same rocks, and with limestone; a bed, or pseudo-stratum, of basalt being in one place interposed, to be considered as an adventitious and accidental rock. The aggregate thickness of the coal-field is here estimated loosely at 500 or 600 fathoms, by others at 180; the beds of shale and clay amounting to 85, those of grit to 70, and of limestone to 20, with one bed of iron-stone. Of the coal-beds, 24 are contained in the first 180 fathoms, and the 25th is under the first limestone, at 250. One foot and a half is an average of the thickness of the coal-beds, and six feet the largest dimension; the limestones are nearly limited to the lower half of the series. It is unnecessary to give more examples of the nature and disposition of the coal in England.

This mineral abounds also in Scotland; and there it presents some differences in the order of succession of the accompanying strata. Though many of them possess the same general characters, they are not all to be identified with the English series; and if some of them are as different as analogous deposites are known to be in different countries, others must apparently be referred to the superior coal, or to that of the lignites. A few examples from that country will tend to throw some light on that which has yet been imperfectly studied; and it will, at the same time, be seen that the positions of the coal-beds are often remarkable as they relate to the primary strata.

In the vicinity of Edinburgh, the usual alternations of coal-beds succeed to the red sandstone, and to a limestone which appears analogous to the mountain limestone of English geologists. But in other places, as in Fife, that limestone is sometimes absent, and the coal series lies immediately on the red sandstone. A local deficiency of this calcareous stratum is easily understood. But the strata which, in England, follow the coal series are here wanting; though small portions of the magnesian limestone, and of the red marl above it, have been found; while, in the south of Scotland, the red marl stratum is known to succeed, being continuous with that of the northern tract of England. At Blackburn, in West Lothian, the coal lies immediately under limestone; as it does in Peebleshire, at Carlops, and Spitalhaugh; and these limestones are supposed to be similar to those which elsewhere appertain properly to the coal series, though the observations are not such as to satisfy us that they are not the magnesian lime-stone. This may probably be safely pronounced as to that in the southern district, though it is always improper to controvert the observations of others without actual examination.

Our geological knowledge respecting the coal of the Glasgow district is very imperfect, but it presents some peculiarities. Near Paisley, the coal lies under a thick bed of limestone, accompanied, as in many other places, by shale, often aluminous, and by sandstone. But as this series has not been studied, it is only safe to remark, that it appears to follow immediately on the red sandstone; and that if it is not succeeded by the actual strata that follow it in England, as has been thought, this limestone also is still likely to be the magnesian.

The coal of Campbelltown, which is presumed to be connected with that of Ireland, lies in a series of three beds, interposed between sandstones, with a very small proportion of shale, forming one distinct and limited basin. Downwards it is followed by a thin bed of red sandstone or conglomerate; and immediately afterwards, by the micaceous schist which bounds the whole tract. No strata seem to lie above this coal-field.

In the Western islands coal is found dispersedly; yet forming parts of a deposite which appears, at some distant time, to have been entire, as I have attempted to show in my work on that district. Abstracting the intruding trap, which it is not necessary here to notice, it is found in Sky, accompanying sandstone and shale, and lying immediately beneath a limestone somewhat resembling the lias of England, being apparently, in some places, interstratified with it. It appears to be followed immediately downwards by another similar limestone, containing gryphites and ammonites, and then by gneiss. A continuation, apparently, of the same deposite is found in Morven; and here, while the whole of the coal series rests immediately on gneiss, without any distinct intermediate rock, as far as it can now be seen, and followed by no superior strata, the geographical position is not a little singular. Owing to the mountainous form of the gneiss, portions of it are found insulated on the summits of primary hills, attaining 2,000 feet in altitude; protected apparently from further immediate waste, by a bed of trap which covers the secondary strata, here, and in the neighbouring island of Mull. Thus the whole series containing coal is included between gneiss and trap; the order being gneiss, limestone, sandstone and coal, trap. The singular obscurity of this coal, from its scattered geographical position, the smallness of its quantity in any one place, the interference of trap-rocks, and the irregularity and deficiency in the succession of the strata, renders it hazardous to decide absolutely on its geological nature; but it is more probable that it ought to be referred to the superior deposites, and not to the true coal series; in which case it will rank with the lowest lignite, and thus, possibly, with the coal of Whitby.

In Sutherland, is found a portion of a coal-field, skirting the eastern shore, and cut off by the sea, which presents also certain peculiarities of character deserving of notice; as they have not hitherto been examined or described. This collection of strata rests immediately on granite; a very minute portion of conglomerate only being in some places interposed; so that, at some points, the beds of coal can be traced within a few inches of the granitic mass; all the usual intermediate strata being absent. The lower beds of the series, where it is most perfect, consist almost solely of limestone, with a few remains of ammonites, pectines, and other broken and obscure shells. But even then the lower beds are wanting in many places; so that the remainder of the series, consisting of alternations of limestone, shale, and sandstone, with carbonized remains of vegetables, reposes on the fundamental rock. The position, as well as the nature of this deposite, mark it as distant from all the collections of coal-strata, even of Scotland; nor is it followed by any superior set of strata. It is probable that it must take its place with the last, and, as such, it belongs to the coal of the lignite formation, described in a preceding paper.

In Ireland, the Connaught coal-strata lie on a body of limestone, of various character; sometimes interstratified with sandstones, and following the lowest red rock of that character. This is succeeded by the usual coal-strata, in that uncertain order which is so prevalent; the series terminating upwards in the shales and sandstones which belong to it. In the Leinster district, a similar limestone reposes on the granite, and is followed by a corresponding succession of beds, but more numerous. But it is unnecessary to enter into any further enumerations; as they afford no instruction to the mere geological student. The information required by the miner is of a far different nature, and must be acquired in all the minuteness of local details.

The red marl sandstone is also said to contain some coal on the continent of Europe, but it does not appear to have been thus found in Britain. What other examples of this substance occur in the higher parts of this series, among which are those in the Kimmeridge clay, a part of the oolite series, those which occur in the sands beneath the chalk, and those which belong to the fresh-water deposites, belong to the lignites, and as such they have formerly come under review.

A very few words on the geography of coal-deposites, as this is found in authors, will not be misplaced; though it is probable that very few of all those which exist in the world are known. It is probable, for example, that the coal-strata of England lie, in the south-eastern tracts, beneath the enormous mass of strata which covers the place where, according to analogy, they should be found. Thus they may equally exist in many other countries, where, from the unfavourable position of the strata, their depth, or the want of external indications, it would be either imprudent or fruitless to seek for them.

I need not repeat that the British islands possess one of the most extensive and valuable deposites of coal yet discovered. That of the Low Countries, including a considerable territory near Liege, is the next in importance in Europe; bearing a considerable resemblance in its essential characters to that of Britain. It is also found at Marienburg in Misnia, at Pecswar in Hungary, at Rottenburg in Silesia, at Bilin in Bohemia, and in Upper Styria. Coal is further found in France, in Languedoc, in Artois, and in Auvergne, as well as in other parts of that country; being worked in forty-seven departments of the empire, and traced in sixteen others. In America, it has been observed in different places, and wrought in some. Among others, may be enumerated Cape Breton and Newfoundland. It is also found in Upper Louisiana, and, more recently, it has been observed by Humboldt, in the valley of Bogotá. One of the most ancient accounts we possess of coal is that of Marco Polo; and it is now better known that it exists in the northern part of the province of Canton, and in that of Kiangsee, as well as in Tartary, where its use was long ago understood. In the Birman empire, it accompanies the celebrated bituminous wells. In the Waldai mountains it is found near Borowitch. Recently, it has been discovered and wrought in New South Wales; and has also been observed in the newly-discovered islands of that sea which is presumed to reach from Baffin's Bay to Behring's Straits. It is probable that nothing is yet wanting in many other parts of the world but knowledge, industry, wealth, and the stimulus of want, to prove that it is a far more common substance than it has hitherto appeared to be.

Now though I have given but a few of the localities of coal quoted by authors, I have suppressed many more, and ought assuredly to suppress some of these. But it is unpleasing to criticize without positive evidence; while it is the duty of a writer to quote those who have the reputation of accurate observers. There can be no question that many of the localities here quoted are instances of the lignite formation, and not of the real coal series, which belongs exclusively to a position beneath the red marl. That may equally be true of others; but we must continue in doubt until more accurate observers have examined these places, under better information respecting the distinctions of lignite and coal. I would willingly have purified a subject which, for want of materials, I must leave in a state of confusion; as it would be dangerous to decide at present as to what belongs to coal, to the real and ancient series, and as to what should be classed with the more recent, or with the lignites. I must here refer back again to that article, as from uncertainty respecting the real character of many examples, I could not avoid mentioning some cases in both.

It remains to say a few words respecting these deposites which have, by some, been improperly distinguished by the name of basaltic coal, sometimes applied also to the lignites in similar positions. The peculiarities of these coal-fields, when they belong to the real series, are highly interesting to the miner; but their geological differences are simple, and easily apprehended. They are analogous to all the phenomena that occur where the trap-rocks intrude among the stratified; nor is there any reason for distinguishing the different appearances, otherwise than from the peculiar influence which the contact of this substance may have exerted on the coal-beds. The basaltic or trap-rocks are found among them, either in the shape of ordinary intersecting veins, or else of parallel ones, forming pseudo-strata; and their presence is accompanied by those effects of disturbance which are now well known, but which belong to the history of the trap-rocks. It is only necessary to remind the reader, that the siliceous schists and indurated sandstones found in such deposites of coal, are the natural consequences of the action of the trap on those strata.

It is well known that there are many varieties in the nature and appearance of coal, considered as a mineral. An arrangement of these having been given in the classification of rocks, it needs not be repeated here. The connexion of its mineral characters, first with lignite, and finally with peat, are important, both in a chemical view, and because the natural history and origin of these substances are connected, and are illustrated by a mutual comparison. That comparison could not now be drawn without repeating part of the history, both of peat and lignite, which has been sufficiently detailed in this Journal and elsewhere to render that unnecessary.

It is yet requisite to offer a few remarks on the organic substances connected with coal; the more so, as to these it appears indebted for its origin. These remains are very predominantly those of vegetables. In some instances shells occur; as is peculiarly remarkable in the case of Derbyshire. While some of these unquestionably belong to fresh water, many are doubtful, and others are thought as certainly to be marine. The genera ammonites, orthocera, mytilus, unio, terebratula, lingula, have been remarked among others; and even the fragment of a fish has been observed. I have elsewhere pointed out the difficulty of pronouncing respecting the exclusive origin of any shell; though there is nothing in the theory of coal that should absolutely exclude marine remains. The most perfect vegetable remains are found in the shales, though they occur also in the sandstone. Yet the coal, too, often contains carbonized wood, or portions of plants, of which the interior organization is sometimes visible; and in some rare instances, such coals so abound in charcoal that it pervades the whole mass; appearing to be rather charred wood than proper mineral coal.

The progress of fossil botany is as yet so imperfect, and those remains are often so broken and obscured, that not much has been done towards their effectual description and arrangement. The reader must be referred to Sternberg, Schlottheim, Brongniart, and others, for that full information which could not here be usefully condensed; and as a mere list of new terms can convey no information, I shall prefer to this a reference to the work of Messrs. Conybeare and Phillips, where a useful abstract is given. It is sufficient to remark, that plants of aquatic habits seem to prevail among them; the predominant remains being those of reedy vegetables, accompanied by others analogous to ferns, and, as it is thought, to palms; with some which appear to possess no exact living analogies. The fluted and the scaly stems and trunks, distinguished by the terms Lepidodendron and Syringodendron, are the most remarkable, as they are the most obscure. The genera seem to be very limited, as far as they can be determined from such imperfect specimens; but it is imagined that the species exceed three hundred, or approach to four. From their predominant characters, many geologists and botanists have considered them as tropical plants: a question requiring an examination that I have not space here to bestow on it.

In the sandstones the most frequent vegetable appearance is that of concretions of sand, bearing the impression of the trunks just noticed; and it is remarkable that, in these cases, while the woody matter of the tree has disappeared, the bark is sometimes converted into coal. Sometimes such trunks are compressed, as if by the superincumbent weight, and are also generally recumbent; but, recently, there have been found near Edinburgh and Glasgow the remains of trees similarly changed, in their erect, apparently original position, with the branches attached. However difficult it may be to account for the absolute disappearance of the wood in these cases, it is a difficulty analogous to that which occurs respecting shells.

Such are the organic appearances connected with coal; all of them bespeaking a terrene, and not a submarine origin, not only for the carbonaceous beds, but for the general, if not the whole, series in which they are found. Their aquatic nature equally indicates the growth of these vegetables in low, moist forests, in marshes, or on the borders of lakes. In the repetition of different strata, we see the successive deposition of earthy matters of different natures, afterwards consolidated into sandstone or shale, or in some cases remaining unconsolidated; while the repetition of carbonaceous beds, no less than the deposition of vegetable strata alternating with the laminæ of the shales, shows that successive generations of plants had followed successive depositions of earth. To the same circumstances of repose, the solution of soluble earths, pressure, the action of water, and, possibly, heat, we must equally attribute the consolidation of the rocks, and the conversion of the vegetable deposites to coal. But as this part of the present subject has been elsewhere examined, I need not recur to it in this place: only remarking that when I name heat, I merely do not assume to deny what I see no reason for admitting as a necessary agent.

Although the vegetable nature of the remains which accompany the coal-strata thus indicate the terrestrial origin of the series, the limestone which lies beneath contains marine remains. These strata, then, have been formed under the sea, which must have become dry; or of which the bottom must have been elevated prior to the deposition of the coal-beds. That the vegetables could not have been transported from their places of growth to the bottom of the sea, is no less proved by the integrity of the most delicate specimens, than by the erect positions of the trunks of trees above-mentioned. Yet, though assuredly formed on the supra-marine land, it is still difficult to account for the great thickness of many deposites, for the frequent alternations of rocks and of vegetable matter. The existence of deep lakes, at some periods and under various modifications, is necessary to explain the phenomena; though we shall find it difficult to comprehend or conceive the exact details of the changes, and the condition of the surface necessary for their production. It does not seem difficult to explain the presence of marine bodies, or even of complete marine strata, if such there really are, as has appeared to many observers. It is merely to suppose an estuary instead of a lake; and it would be easy to show that the mode of deposition, and the alternation of substances, are at present similar in both. In the history of the fresh-water formations there are analogous difficulties; yet, in this case, the necessity of similar changes is equally demonstrated. The one difficulty illustrates the other, and confirms the truth of the general views: hereafter, the accumulation of more parallel instances, under different modifications, may lead to an easy solution of that which now appears so difficult to explain.

From the marine origin of the strata above the coal series, it is an obvious consequence that those parts of the surface which were once above the water, and which received the deposites of land vegetables, have been subsequently immersed beneath an ocean, during that period of unknown, but great duration, requisite for the accumulation of such enormous masses. The last change restored to the surface of the earth, and often to great elevations, those strata which had once before occupied it; accompanied by all those which are found above it, and by many of those which lay below. Such changes could not have taken place without violence and derangement; and to them we must attribute the fractures, dislocations, and flexures, of the coal-strata, no less than those of the other rocks which have undergone analogous revolutions.

The economical management of coal-strata, and the art of mining them, form a subject foreign to the objects of this paper; and they may be found in numerous treatises written for that end.

Art. VIII.—*Remarks on Local Influences on Compasses, on the Coast of Northumberland, and among the Group of Farn Islands.*

[To the Editor of the *Quarterly Journal.*]

Sir,

During my survey of the Farn Islands several years ago, having observed the compass very peculiarly affected when placed on the margin of the western cliff of Farn, or St. Cuthbert's Island, I was induced to try an experiment there last year; the result of which, it has lately been suggested to me, might be acceptable to the readers of the *Journal of Science*, whether considered as one among the many accumulating facts relative to local influences on the magnetic needle, or, in a practical point of view, as connected with the actual navigation of a group of islands on our own coast.

It is known that the laws of action of permanent magnets and simple iron are very distinct; but the distance at which local influences on the magnetic needle commence or terminate is by no means so clearly ascertained. This circumstance, and the variety of curious effects noticed by the navigators of high latitudes, together with the fact that the magnetic needle deviated six points from its true direction when the compass-box was placed on the margin of the western cliff of Farn Island, induced me to endeavour to ascertain whether the ferruginous or other substances deposited in the strata of this singularly-diversified coast might affect the compasses of ships in such a manner as to render them unsafe to steer by, when used for the navigation of the intricate passages among the group of Farn Islands.

The strata of that part of the Northumbrian coast where the experiment was tried consist of trap, limestone, grit, shale, claystone, and felspar, variously interspersed with quartz, carbonate of lime, particles of iron pyrites, sandstone, iron-glance in veins and in crystals, calcareous and heavy spar, &c., together with a quantity of ferruginous matter, such as is usually deposited in the fissures or interstices of the basaltic formations, as well as particles of a similar nature mingled with the sand on the coast, and which were attracted by the magnet.

Having established a direct line of bearing from Bambro' Castle to Budle Point, a distance in which the general magnetic phenomena could not be supposed to vary materially, I placed several poles, at certain distances from each other, in that line, and on the various strata. I then observed the bearings of the direct line from every station with several magnetic needles, elevated by means of a tripod four feet and a half above the rock, sand, or soil, where the poles stood.

The operation was simple, and the result decisive; showing that although discordances in the bearings taken from each station did occur, yet as they did not exceed a degree, and as no ship should approach so near to a rock as four feet and a half, it appeared obvious that neither the minerals nor the ferruginous depositions in the different strata would cause the compass of any ship, in the proper channels among these islands, to deviate in such a manner as to render it unsafe to steer by; and this result was corroborated by bearings which I took of fixed objects from stations on the Longstone, Farn Island, Bambro' and Holy Island Castles, &c.: and notwithstanding the compass deviated six points when the box was placed upon the cliff of Farn Island, as before stated, yet when elevated on the tripod at a very inconsiderable distance to the eastward of the cliff, the bearings agreed with those taken in the same line of direction on the other islands.

Although, in common with other persons who have given some attention to the nature of local influences on the magnetic needle, I certainly anticipated the result which occurred; yet considering that one fact may be of more real utility than a volume of suppositions, I have, under that idea, been persuaded to communicate the foregoing circumstance.

I am, Sir,
Your very humble servant,
EDWARD J. JOHNSON.

London, December 1, 1826.

ART. IX.—*Proceedings of the Royal Society.*

THE Meetings of this Society were resumed for the season on *Thursday*, *Nov.* 16, when the President announced that the Apartments formerly occupied by the Commissioners of the Lottery had been assigned to the use of the Royal Society; he also informed the meeting of the resignation of Mr. Brande, one of the Secretaries of the Society.

Lieutenant-Colonel D. Denham, Captain W. H. Smyth, R. N., and Nicholas Brown, Esq., were respectively admitted Fellows of the Society.

The Croonian Lecture, by Sir Everard Home, Bart., V. P., was read. The subject of it was the *Generation of Oysters and Muscles.*

November 23.—Charles Bell, Esq., was admitted into the Society; and Messrs. Bouvard, Dulong, and Chevreul, were elected Foreign Members of the Society.

A paper by Lieutenant-Colonel Millar was communicated by R. J. Murchison, Esq., describing a Percussion Shell intended to be fired from a common gun.

Thursday, November 30.—This being St. Andrew's Day, the Society, according to usual custom, held their Anniversary Meeting.

The President took the Chair at eleven in the morning, and began the business by reading the lists of the Fellows who had been admitted, and those they had lost by death, since the last Anniversary. Among the Foreign deceased Members he mentioned, with particular notice, Scarpa, the celebrated anatomist of Pavia; and Piazzi, the discoverer of the planet Ceres; these Gentlemen, said the President, died according to the ordinary course of nature, in old age, having enjoyed a glory which in no respect disturbed their repose. Among the Home Members, he dwelt at some length on the loss of Mr. Taylor Combe, and Sir Stamford Raffles; the last of whom he eulogised as a most disinterested and liberal contributor to Natural History. "Occupying high situations," said the President, "in our Empire in the East, he employed his talents and his extensive resources, not in the exercise of power or the accumulation of wealth, but in endeavouring to benefit and to improve the condition of the natives, to fix liberal institutions, and to establish a permanent commercial intercourse between the Colonies in which he presided and the Mother-Country; which, while it brought new treasures to Europe, tended to civilize and to improve the condition of the inhabitants of some of the most important islands of the East. Neither misfortune nor pecuniary losses damped the ardour of his mind in the pursuit of knowledge. Having lost one splendid collection by fire, he instantly commenced the formation of another; and having brought this to Europe, he made it, not private, but public property, and placed it entirely at the disposition of a new Association for the Promotion of Zoology, of which he had been chosen President by acclamation. Many of the Fellows of this Society can bear testimony to his enlightened understanding, acute judgment, and accurate and multifarious information; and all of them must, I am sure, regret the premature loss of a man who had done so much, and from whom so much more was to be expected, and who was so truly estimable in all the relations of life."

After stating the foundation of the Royal Medals, which had been announced to the Society at their Anniversary Dinner last year, by the Right Honourable the Secretary of State for the Home Department; and which, said the President, having been offered in the true spirit of Royal munificence, had been completed with an exalted liberality worthy of the August Patron of the Royal Society—being intended to promote the objects and progress of Science, by awakening honourable competition among the Philosophers of all countries; he proceeded to state, that the Council had awarded the first Prize to Mr. John Dalton, of Manchester, for the development of the chemical theory of definite proportions, usually called the Atomic Theory, and for his various other labours in Chemical and Physical Science. He mentioned the names of Dr. Bryan Higgins, Mr. William Higgins, and Richter, as having contributed something towards the foundations of this part of science; but, he said, as far as can be ascertained, Mr. Dalton was not acquainted with any of their publications; and whoever considers the original tone prevailing in all his views and speculations, will hardly accuse him of wilful plagiarism. But, let the merit of discovery be bestowed wherever it is due, and Mr. Dalton will be still pre-eminent in the History of the Theory of Definite Proportions. He first laid down, clearly and numerically, the doctrine of multiples, and endeavoured to express by simple numbers the weights of the bodies believed to be elementary. The first views, from their boldness and peculiarity, met with but little attention; but they were discussed and supported by Drs. Thomson and Wollaston; and the scale of chemical equivalents of the latter gentleman separates the practical part of the doctrine from the atomic or hypothetical part, and is worthy of the celebrated author. Gay-Lussac, Berzelius, Prout, and other chemists, have added to the evidence in favour of the essential part of Mr. Dalton's doctrine; and for the last ten years it has acquired, almost every month, additional weight and solidity.

The President begged to be understood, that it was the fundamental principle that he was contending for, and not Mr. Dalton's particular statement of the nature of bodies, and the numbers representing them, given in Mr. Dalton's *New System of Chemical Philosophy*. In this, he said, as a first sketch, many of the opinions are erroneous, and the results incorrect, and they are given with much more precision in later authors. It is in the nature of physical science that its methods offer only approximations to truth; and the first and most glorious inventors were often left behind by very inferior minds, in the minutiæ of manipulation, and their errors enable others to discover truth.

"Mr. Dalton's permanent reputation," continued the President, "will rest upon his having discovered a simple principle universally applicable to the facts of Chemistry, in fixing the proportions in which bodies combine, and laying the foundation for future labours respecting the sublime and transcendental part of the science of corpuscular motion. His merits in this respect resemble those of Kepler in Astronomy. The causes of chemical change are as yet unknown, and the laws by which they are governed; but in their connexion with electrical and magnetic phenomena, there is a gleam of light pointing to a new dawn of science. And may we not hope," said the President, "that in another century, chemistry having, as it were, passed under the dominion of the Mathematical Sciences, may find some happy genius, similar in intellectual power to the highest and immortal ornament of this Society, capable of unfolding its wonderful and mysterious laws? I trust," said the President, "you will allow the justice of the decision of your Council, which has claimed for our countryman the first testimony of Royal benevolence. There is," he said, "another motive which influenced them, and which, I am sure, will command your sympathy. Mr. Dalton has been labouring for more than a quarter of a century with the most disinterested views. With the greatest modesty and simplicity of character, he has remained in the obscurity of the country, neither asking for approbation, nor offering himself as an object of applause. He is but lately become a Fellow of this Society; and the only communication he has given to you is one, compared with other works, of comparatively small interest. Their feeling, therefore, on the subject is perfectly pure. I am sure he will be gratified by this mark of your approbation of his long and painful labours. It will give a lustre to his character which it fully deserves. It will anticipate that opinion which posterity must form of his discoveries, and it may make his example more exciting to others in their search after useful knowledge and true glory." The President then announced, that the second Medal on the Royal foundation was awarded to James Ivory, M. A., for his papers on the Laws regulating the Forms of the Planets, on Astronomical Refractions, and on other Mathematical illustrations of important parts of Astronomy. He then entered into a particular view of the merits of the papers communicated by Mr. Ivory to the Royal Society, being seven in number, on the most difficult and abstruse points of science; and quoted M. de Laplace as having borne testimony to some of their merits. After paying some high compliments to Mr. Ivory for his disinterested pursuit of objects of science which have no immediate popularity, and which are intelligible only to a few superior minds, and stating that all the Mathematicians of the Council were unanimous in claiming this reward for him; he said he felt satisfaction in the hope that this reward might, as an example, renovate the activity of the society, which for so many years was pre-eminent in this department of science, and that it might return, *veteris vestigia flammæ*, with new ardour to its too long neglected fields of glory.

"Whether," said the President, "we consider the nature of the Mathematical Science or its results, it appears equally among the noblest objects of human pursuit and ambition. Arising a work of intellectual creation, from a few self-evident propositions on the nature of magnitudes and numbers, it is gradually formed into an instrument of *pure reason*, of the most refined logic, applying to and illustrating all the phenomena of nature and art, and embracing the whole system of the visible universe. And the same calculus measures and points out the application of labour, whether by animals or machines—determines the force of vapour, and confines the power of the most explosive agents in the steam-engine—regulates the forms of structures best fitted to move through the waves—ascertains the strength of the chain-bridge necessary to pass across arms of the ocean—fixes the principles of permanent foundations in the most rapid torrents; and, leaving the earth filled with monuments of its power, ascends to the stars, measures and weighs the Sun and the Planets, and determines the laws of their motions; and even brings under its dominion those cometary masses that are, as it were, strangers to us, wanderers in the immensity of space; and applies data gained from the contemplation of the Sidereal Heavens to measure and establish time, and movement, and magnitudes, below."

In announcing the award of the Medal on Sir Godfrey Copley's Foundation, for this year, the President stated, that it had been given to James South, Esq., for his paper on the *Observations of* *the apparent Distances and Positions of* 458 *Double and Triple Stars*, published in the present volume of the *Transactions.*

"The researches and observations of Double Stars," said the President, "recommended by Mr. Mitchell, were pursued at first by Sir William Herschell, in the hope of discovering the Parallax of the fixed Stars; and, afterwards, when his discoveries opened new views of the nature of the Sidereal Heavens, with the hope of ascertaining whether Systems did not exist among the fixed stars bearing relation to the planetary world, and demonstrative of the laws of gravity. These researches, pursued, for many years, by Sir William Herschell, were, after his death, continued by his son, Mr. Herschell, and Mr. South, with an instrument adapted for the purpose by Mr. Troughton. The combined observations, according to Mr. South, establish several important points—such as occultations of fixed stars by each other, proper motions of fixed stars, and revolving systems, in which two stars perform to each other the office of sun and planet, several of them having revolutions which may be assigned from fifty-three to one thousand four hundred years." After giving a pretty extensive account of Mr. South's conclusions, the President said: "When the importance of an acquaintance with the position of the fixed stars in the Heavens is considered, on the accurate knowledge of which all our data in refined astronomy, and many of those in practical navigation, depend; and when the new and sublime views of the arrangements of Infinite Wisdom, in the Starry Heavens, resulting from these inquiries, are considered, the Society will, I am sure, approve of this vote of the Council. Mr. South procured his instruments at a great expense, and employed them at home, and carried them abroad, trusting entirely to his own resources. He has pursued his favourite science in the most disinterested and liberal manner, and has communicated all his results to this Society. There is another reason which may almost be considered as personal. Whoever has seen the methods in which observations of this kind are conducted, must be aware of the extreme fatigue connected with them—of the watchful and sleepless nights that must be devoted to them—of the delicacy and manipulation they require—and of the sacrifices of ease and comfort they demand."

In presenting the Medal to Mr. South, the President referred to it as the oldest mark of distinction which the Society had to offer, and which was more valuable from the illustrious names to which it had done honour, and the great and extraordinary advances in natural knowledge with which it had been connected. "Receive it," he said "(to use a metaphor taken from the Olympic Games), as the honorary Olive Crown of this Society; and may it be a stimulus to induce you to pursue and persevere in these highly-interesting astronomical researches, and steadily to apply your undivided attention to them, secure that posterity will confirm their utility, and that the glory resulting from them will be exalted by time!"

The Society then proceeded to the election of the Council and Officers, when, on examination, the following was found to be the state of the lists.

Eleven Members of the Old Council to remain Members of the New Council:—

Sir Humphry Davy, Bart., President.
John Barrow, Esq.
The Lord Bishop of Carlisle.
D. Gilbert, Esq., V. P., Treasurer.
John F. W. Herschel, Esq., M. A., Secretary.
Sir Everard Home, Bart., V. P.
Captain Henry Kater.
John Pond, Esq., A. R.
James South, Esq.
W. Hyde Wollaston, M. D., V. P.
T. Young, M. D., Foreign Secretary.

The Members elected into the Council:—

John Abernethy, Esq.
Charles Babbage, Esq., M. A.
Captain Francis Beaufort, R. N.

Robert Brown, Esq.
John George Children, Esq.
Charles Hatchett, Esq.
A. B. Lambert, Esq.
William, Viscount Lowther.
George Pearson, M. D.
William Prout, M. D.

The following were elected Officers of the Society:—

Sir Humphry Davy, Bart., President.
D. Gilbert, Esq., V. P., Treasurer.
John F. W. Herschel, Esq., M. A., Secretary.
John George Children, Esq., Secretary.

The Society then adjourned to dine together at the Crown and Anchor Tavern. Here we were sorry to observe the absence of the President, on account of illness.

ART. X.—*Proceedings of the Horticultural Society.*

OUR reports upon the proceedings of this Society have for some time been discontinued; we trust that the arrangements which have been made will hereafter secure a regular statement for our pages.

Tuesday, October 3, 1826.—The most remarkable subject of exhibition was a fruit of the Mango-tree, *Mangifera indica*, which had been produced for the first time in Europe in the conservatory of the Earl of Powis. Its figure was oval, its length about four inches and a half, its colour bright yellow, deeply stained with crimson, and its flavour quite as luscious as it is said to be in the climates under which it is produced spontaneously. It was stated that a considerable number of similar specimens had been borne by the same tree; and that the greatest difficulty which had been supposed to exist in the cultivation of plants in an artificial atmosphere had been thus overcome. The same nobleman exhibited fresh fruit of the Yellow Rose Apple, and the Purple Guava. It is a remarkable fact that this tropical dessert had been procured in a wooden hot-house of common construction, warmed by heated water conveyed through it in pipes, and subjected, during the whole period of maturation of the fruit, to a very unequal temperature, owing to repairs of the house at that time taking place.

Plants were placed upon the table, in flower, of a new and very pretty species of *Gilia;* a hardy annual, the seeds of which had been collected in the vicinity of the river Columbia by Mr. David Douglas. This enterprising individual was engaged by the Society to proceed to the north-west coast of North America, for the purpose of making discoveries in Botany, and other branches of Natural History. By permission of the Hudson's Bay Company, he sailed, in July 1824, on board a Company's ship, for the mouth of the river Columbia. After touching at Rio, making a short but very advantageous stay at the island of Juan Fernandez, and visiting the Gallipagos, he reached his destination in the spring of 1825. During his residence in the country he has succeeded most completely in performing every object of his mission. The number of new plants, of the most singular forms, of which he has already sent home dried specimens and seeds, is greater than could possibly have been anticipated. Among those which are already growing in the Society's Garden, besides the plant now exhibited, are the *Gaultheria Shallon*, at once one of the most beautiful and rarest of known plants; the *Rubus spectabilis* of Pursh; both the species of *Brodiæa*, before scarcely known, except by the specimens collected by Mr. Menzies, in Vancouver's voyage; various undescribed species of *Lupinus*, and a vast number of other plants of the greatest interest, both to Horticulture and to Botanical Science. In the collection are nearly the whole of Lewis' and Clarke's plants, seeds of the greater part of which have already reached England in good condition. The last despatches which have been received from Mr. Douglas were dated April 13, 1826, at which time he was about to proceed to the Rattle Falls in the interior. At that time he had learned from the Express from Hudson's Bay, that they had met with Captain

Franklin's party on Cumberland Lake, in their way to Bear Lake, their winter-residence. The gentleman who commanded the Express party had spent the last five years upon the Mackenzie river, and had learned from the natives, to whom he was well known, speaking their language fluently, " that there is a river to the west of the Mackenzie, and parallel to it, equal to it in size, and running into the sea near Icy Cape; that at the mouth of the said river, on an island, there is a trading establishment, where ships come in the summer; that the traders * wear large beards, and are very wicked, having hanged several of the natives to the rigging of their ships; in consequence of which they have fallen into much disrepute." Mr. Douglas's informant had procured from the natives several of the articles in which these strangers trade, " among which were Russian coins, and some metal combs, of coarse workmanship," (one of which has been received in England;) " that these same strangers had also for barter, large iron pots, holding four, five, and six gallons each, of very different workmanship from those in which the British Fur Company trades." Some of these had been seen. By the same intelligent person, a Mr. Mac Leod, it was stated that the sea to the west of the Mackenzie is open after July, and that little exertion would be required to reach Icy Cape by land. Mr. Douglas had also gained intelligence of a gigantic species of Pinus, growing to the south of the Columbia. The trees are stated to measure from twenty to fifty feet in circumference, and from one hundred and seventy to two hundred and twenty feet in height; to be nearly destitute of branches till within a short distance of the summit, which divides into a fine spreading head; the trunks are reported to be remarkably straight, the timber fine, and abounding in resin. By the Umtqua Indians, in whose country the trees grow, the seeds are dried, pounded, and baked into a sort of cake. Mr. Douglas had procured one cone of this prodigious tree, which was sixteen inches and a half long, and ten in circumference at the thickest part. It is probable that this Pine is the same as that spoken of by

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* Quære, Russians of Vasilieff's Expedition in 1822?

Lewis and Clarke (p. 422), as having given so grand a feature to the landscape near the mouth of the Clatsop river in the country of the Killamucks.

Tuesday, *October* 17.—The President announced from the Chair, that a vacancy existing among the Honorary Members of the Society, the Council had determined upon proposing Robert Brown, Esq. to fill the vacancy. He was unanimously elected.

Tuesday, *November* 7.—A paper was read upon the species of the *Genus Hoya*, by Mr. James Traill. Several new species were noticed, and descriptions given of those in cultivation.

Mr. Wells of Redleaf exhibited some fine specimens of different varieties of Chestnuts, which had ripened in his garden near Tunbridge. These were so fully matured, as to give rise to hopes that we may, one day, become independent of the South of Europe for this important winter-fruit.

Tuesday, *November* 21.—It was announced from the Chair that Mr. John Turner, the Assistant-Secretary, having abused the confidence reposed in him by the Society, by not accounting for various monies which had been received by him, had been dismissed from the service of the Society.

A paper was read by Mr. John Hay, suggesting improvements in the construction of Flued Walls for Gardens. This kind of wall is not much used in the southern parts of the kingdom; but to the natives of our northern counties, many of the productions of the Garden are almost unattainable without it. Mr. Hay remarked that the common mode of constructing these walls with four horizontal flues was objectionable for various reasons, with which cultivators are practically too well acquainted. Mr. Hay proposes to remedy such inconveniencies by throwing heated air into the whole cavity of the wall, which is to be built hollow for the purpose; and to avoid the sudden chilling of the wall, by a current of cold air, as in common cases, he fixes dampers at either extremity of the flues, by which the heated air is confined, instead of being suffered to escape rapidly.

A splendid show of Chinese Chrysanthemums, in flower, from the Society's Garden, was placed at the lower end of the room. The brilliant colours of these plants, and the facility with which they are cultivated, will soon make them so common, that our cottagers' gardens will become as gay in the months of November and December, as the Chinese Rose has made them during all the spring and summer. Such objects as these, which add universally to the comforts and luxuries of the country, and which may, therefore, be justly termed of national importance, are those to which the power and attention of this and all similar public bodies cannot be too forcibly directed. There are now fifty-two varieties of the Chrysanthemum in this country, most of which have been introduced by the Society within the last few years.

Art. XI.—*Proceedings of the Medico-Botanical Society.*

Friday, 13*th October*, 1826.

This Society held its first meeting. The President, Sir James M'Grigor, K.T.S., President, in the chair. The Director (Mr. Frost) delivered the annual oration, in which he congratulated the Society on the rapid advance it had made during the last session; and he also informed the meeting that their President, the Director-General of the Army Medical Board, had lately ordered, that " no person shall be admitted to an examination to qualify him to practise in the Medical Department of the Army, without having attended, amongst other branches of science, lectures on Botany, for six months, the salutary effects of which regulation would in a few years demonstrate its utility."

The Chev. Castille, K.C.S., His Catholic Majesty's Consul-General in London, was admitted a member of the Society.

Letters from Baron Humboldt, Baron Jacquin, M. Desfontaines, and Baron Ferussac, were read. A communication from His Britannic Majesty's Vice-Consul for Guatemala. M. Shenly, was read, and the meeting adjourned.

Friday, 10*th November.*

A Paper entitled *Observations on the Plant which affords the Ipecacuanha of the Shops*, by John Frost, Esq., was read. The object of this communication was to point out the botanical characters of the plant which yields the article in question; it having been referred to the genera Euphorbia, Bycetria, Cephaelis, Asclepias, and Callicocca; the author then entered into a detail of the various opinions respecting it, and went on to show the probability of its being procured from a plant named "Ionia Ipecacuanha."

Henry Williams, Esq., of Sierra Leone, was admitted a member of the Society.

The following gentlemen were elected into the Society:

Michael Macturch, Esq., M.A.
John Fleming, Esq., M.P., F.R.S.
Benj. Hawes, Jun., Esq.
Thomas Bell, M.D.
Rev. Stephen Isaacson, M.A.
C. H. Persoon, P.D. Corresponding Member.

Notice was given from the chair, respecting the papers to be sent in as essays for the gold and silver medals, and the meeting adjourned.

Friday, 8*th December.*

A Paper entitled *Observations on the Properties of Menyanthes Trifoliata*, by J. P. Yozy, Esq., Member of the Society, was read, which gave an account of the use of the plant, for the purpose not only of pharmacy, but its application to the arts.

The following gentlemen were elected into the Society:

Wm. Lloyd, Esq.
Wm. Symmons, Esq.
I. J. Vandercom, Esq.
Whitelaw Ainslie, M.D.
Arther Biggs, Esq., F.L.S.
Mons. Feuillet, Corresponding Member.

The meeting then adjourned.

Art. XII. ANALYSIS OF SCIENTIFIC BOOKS.

1. *Philosophical Transactions of the Royal Society of London, for the year* 1826. Part the Third.

i. *On the Coagulation by Heat of the Fluid Blood in an Aneurismal Tumour. By* Sir Everard Home, Bart., V.P.R.S.

A good deal of fault has been found with Sir Everard in several contemporary publications for his management of this case. In our opinion he has pointed out a mode of treatment which deserves some consideration, as applicable with a chance of success to diseases either hopeless in themselves, or requiring treatment infinitely more barbarous and equally uncertain. The following is an abridgment of his account. In a case of aneurism in the external iliac artery, he tied the femoral artery below the sac, but finding that neither the pulsation nor the increase of the tumour were arrested, he attempted to coagulate the blood contained in it by plunging into it a long and sharp needle, the protruding end of which being heated in a spirit lamp, its good conducting power enabled the end in the tumour to become hot enough to effect the coagulation of the blood. After several such applications, modified a little according to circumstances, the pulsation in the tumour ceased, it felt solid, and the progress of the aneurism appeared to be stopped. All went on pretty well for some days, when coldness of the foot and leg came on, the parts lost their sensibility, mortification ensued, and the patient died in about six weeks, in consequence of the pressure of the tumour upon the surrounding parts. "From the statement that has been given," says Sir Everard, "I trust that I have fully proved that the heated acu-punctorium can coagulate the fluid blood contained in the aneurismal sac; and in doing so is not productive of any local or constitutional symptoms that should prevent its being employed for that purpose." Some experiments on the effect of heat on blood, on buffy blood, and three plates from drawings by Bauer, are associated with this paper.

ii. *On the Mathematical Theory of Suspension Bridges, with Tables for facilitating their Construction. By* Davies Gilbert, Esq., V.P.R.S., &c.

The Author's attention was first directed to a consideration of suspension bridges and of the catenary curve on which their theory depends, when the plan for making such a communication across the Menai strait was submitted to the commissioners appointed by parliament for improving the roads and bridges of Wales. It then appeared to Mr. Gilbert, that the proposed depth of curvature was not sufficient for ensuring such strength and permanence as would be consistent with the due execution of a great national work. This opinion he advanced as a member of the commission, but wishing to take upon himself the full responsibility for such increase of expense as was necessarily occasioned by enlarging the curvature, he printed in this Journal (see vol. x. p. 230) some observations confirming his opinion. The interval between the points of support and the roadway was consequently augmented to fifty feet, and the bridge may now be said to possess that full measure of strength which experience has established as requisite for works of iron not perfectly at rest.

As bridges of suspension are coming into very general use, Mr. Gilbert has rendered the public no unimportant service in the present communication, in which he has expanded into tables the formulæ from which his approximations given in this Journal were derived, and has added to them other formulæ and tables for the catenary of equal strength: "a curve," he observes, "not merely of speculative curiosity, but of practical use where a wide horizontal extent may chance to be combined with natural facilities for obtaining a correspondent height for the attachments."

iii. *On Magnetic Influence in the Solar Rays. By* I. H. Christie, Esq., F.R.S., &c. &c.

The fact which Mr. Christie *proves*, as we think, though somewhat more verbosely than necessary, in this paper, is one of considerable curiosity and importance; namely, that a magnetic needle comes to rest more quickly when vibrated and exposed to the rays of the sun, than when vibrated in the shade, and this entirely independent of any mere effect of heat or change of temperature. When the needle was *shaded*, he could easily mark the *fiftieth* vibration; when it was *exposed*, he could not distinguish beyond the *fortieth*. This is one experiment; but many are detailed, all uniting to show a tendency in solar radiant matter to check the vibrations of the magnetic needle. Magnetism is at present in a very unsettled state; its history is full of contrarieties, mysteries, and difficulties. Mr. Christie is a very diligent and successful experimentalist, and we trust that he will not suffer himself to be jostled out of the path he is pursuing, nor quit the steady light of experiment to follow the meteoric illusions of pseudo-mathematical speculations.

IV. *On the mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the resulting Compounds. By* Mr. H. Hennell, Chemical Operator at Apothecaries'-hall.

This is an interesting paper, and we are sorry to observe that it is the only one on practical chemistry in the volume. It would have been more perfect and satisfactory than it is, had the common courtesy been shown either to the author or to the communicator, of allowing him to revise and correct the proof-sheets before their ultimate consignment to the press; but as this usual favour was withheld, the author has been disappointed in obtaining a few copies for private circulation—deprived of the opportunity of illustrating his conclusions by the aid of results obtained since the communication of the paper—and of removing some trifling errors which are now permanent. The nature of that singular body which has long been known in pharmacy under the name of *oil of wine:* the saturating power in respect to sulphuric acid of a peculiar compound of carbon and hydrogen: and the true composition of the salts, called by Vogel *sulphovinates*, are among the facts explained in this paper.

Oil of wine is to tests a perfectly neutral compound, and when mixed with a cold solution of muriate of baryta occasions no change; but if the liquids be boiled together, sulphate of baryta is thrown down. In the same way, if oil of wine and solution of potassa be boiled down to dryness and the residue ignited, sulphate of potassa is the result. Such methods enabled Mr. Hennell to determine with precision the quantity of sulphuric acid furnished by the decomposition of oil of wine, and the mean result amounted to 38 *per cent.* By other experiments he shows that the remaining elements of oil of wine are carbon and hydrogen in the same relative proportions as they exist in olefiant gas: they consequently amount to 62 *per cent.*

By gently heating oil of wine and water with carbonate of baryta, a considerable portion of the latter was dissolved with effervescence; and on filtering the solution, and adding carbonate of potassa, carbonate of baryta was thrown down, and the supernatant liquor yielded on evaporation thin lamellar crystals, very soluble, burning like ether when heated, and leaving an acid sulphate of potassa. These crystals were submitted to a careful analysis, from which it appears that, independent of a portion of water, they consist of one proportional of potassa, two of sulphuric acid, four of carbon, and four of hydrogen; so that in these salts, which, as the author elsewhere shows, are identical with the *sulphovinates*, the four proportionals of carbon with four of hydrogen, seem to exert a saturating power over one proportional of sulphuric acid.

v. *On a Method of expressing by Signs the Action of Machinery. By* Charles Babbage, Esq., F.R.S.

In this paper Mr. Babbage explains, with his usual clearness and ingenuity, a method of denoting by *signs* almost all those circumstances which occur in the motion of machinery, and he has selected, as illustrations of the application of his method, the common eight-day clock and the hydraulic ram. Without the annexed plates, however, we should in vain endeavour to give an intelligible idea of the means which he pursues, and even then we apprehend few of our readers would be at the pains of following us up.

vi. *On the Parallax of the fixed Stars. By* J. F. W. Herschel, Esq., M.A., S.R.S.

In this paper Mr. Herschel suggests a method of investigating parallax, more delicate and at the same time more simple than any hitherto proposed; and although he does not follow it out into actual practice, nor, as we are sorry to observe, intend so to do, we trust that it will be pursued by some practical astronomer, and lead to something more satisfactory than all the contradictory statements hitherto published upon this subject. For the details of his method, our limits oblige us to refer to the author's original paper.

vii. *A Formula for expressing the Decrement of Human Life;* in a Letter to Sir E. Hyde East, Bart., &c. *By* Thomas Young, M.D., &c.

Dr. Young justly remarks that the investigation of the laws by which the general mortality of the human species appears to be governed is of equal importance to the statesman, the physician, the natural philosopher, and the mathematician, and proceeds to the details of an inquiry, into which he has entered, with a view of appreciating and reconciling the discordant opinions that have been advanced respecting the comparative mortality of mankind at different times and under different circumstances. He allows that of late years, whether from the protective effects of vaccination in infancy, or from the increase of the comforts of the poorer, and of the temperance of the more affluent classes of society, or from the simplification of the practice of physic and surgery, there is a decided increase in the mean duration of life in many parts of Europe. At the same time he adds, that it is probable this improvement has been exaggerated, partly on account of the limited description of the persons on whom the observations have been made, and partly from an erroneous opinion respecting the profits of certain establishments which have been attributed to the employment of too low an estimate of mortality, while they have, in fact, been principally derived from the high rate of interest which the state of public credit has afforded.

Dr. Young then proceeds to the details of his subject, to which we must refer those who are interested in the abstruse inquiry which follows, and in the correction of some curious errors into which several writers upon it have inadvertently fallen.

viii. *Account of an Experiment on the Elasticity of Ice. By* B. Bevan, Esq.

This paper still leaves the question unsettled.

ix. *Results of the Application of* Capt. Kater's *Floating Collimator to the Astronomical Circle at the Observatory of Trinity College Dublin; and Remarks relative to those Results. By* the Rev J. Brinkley, D.D., &c.

Of the floating collimator invented by Capt. Kater, we have given a description upon a former occasion (vol. xxxix. p. 301). This communication is a testimonial of its usefulness and importance, from authority which is justly considered as high.

x. *On the Means of facilitating the Observation of distant Stations in Geodætical Operations. By* Lieut. Thomas Drummond, of the Royal Engineers.

Our readers will find some account of the ingenious contrivance described in this paper in our Twenty-first volume (p. 332). Quicklime, when intensely heated, has the singular property of giving out a most vivid light. The author, therefore, availed himself of a ball of that substance of the size of a pea, so placed in the focus of a parabolic mirror as to admit of being intensely ignited by the flames of several spirit lamps directed towards it by as many streams of oxygen gas issuing from separate blow-pipes. The result was, a light of from 75 to 90 times the intensity of that of a well-trimmed Argand lamp—a light so vivid as only to be borne for a moment by the unprotected eye. The practical applications of this method are contained, with other useful particulars, in Mr. Drummond's paper.

xi. *On Burrowing and Boring Marine Animals. By* Edward Osler, Esq.

This paper would perhaps have been more appropriate to the Linnæan than to the Royal Society; the latter, however, have done well to publish it, for it is a work of experience and observation. The author's chief object is, to explain the mechanism by which the boring and burrowing shell-fish form their habitations: this he does, minutely and perspicuously, by the aid of several plates, and he relieves the dryness of mere anatomical details, by the interspersion of such physiological remarks as the subjects suggest.

The animals in question generally burrow and bore by mere mechanical means; by a more or less perfect rotatory motion; but the saxicava is a curious exception, and Mr. Osler shows, by very satisfactory evidence, that these and some other animals must avail themselves of other means of perforation: that these means are chemical, appears by their uniform exertions upon calcareous matter, and by siliceous bodies intercepting their progress: the nature of the solvent, however, he has in vain endeavoured to detect—he could find no free acid, nor any excess of lime in the water in which they had been preserved; but then he observes, that they will not work in confined vessels and deprived of food, and fairly enough presumes, that the solvent, whatsoever it is, is secreted only when its agency is required.

xii. *An Account of some Experiments relative to the Passage of radiant Heat through Glass Screens.* *By* the Rev. Baden Powell, M.A., F.R.S.

Mr. Powell here examines the result of an experiment of De la Roche, respecting the direct transmission of terrestrial radiant heat through glass. The case is that of a second glass screen interposed between the first and the thermometer, which occasions much less proportional diminution of transmission than the first screen: hence it was supposed that the particles of radiant heat had suffered polarisation. Mr. P. here verifies De la Roche's experiment, but rejects his theory, finding, from the temperatures acquired by the screens, that the effect might be referred to a secondary radiation from the screen. The author concludes this paper with a refutation of Mr. Ritchie's notion, that simple heat radiates through very thin glass when transparent, but not when opaque.

xiii. *The Bakerian Lecture; on the Relations of Electrical and Chemical Changes.* *By* Sir Humphry Davy, Bart., P.R.S.

He who has attentively studied the Bakerian Lectures published by this author in the *Philosophical Transactions* some twenty years ago, will peruse the present production with some degree of disappointment, more especially if he has made himself master of the researches of Becquerel, and of Walcker, upon the subjects of which it treats. Of these Bakerian Lectures, the first (*Phil. Tran.* 1807,) is a masterpiece of electro-chemical research: it not only abounds in important discoveries, but in a variety of new views and new suggestions, many of which have been seized upon and appropriated without the smallest acknowledgment, by certain eminent philosophers now extant: the conclusion, for instance, is there drawn, *that the combinations and decompositions by electricity are referrible to the law of electrical attractions and repulsions;* and the hypothesis is advanced, *that chemical and electrical attractions are produced by the same cause* ACTING IN ONE CASE ON PARTICLES, IN THE OTHER ON MASSES; *and that the same property under different modifications is the cause of all the phenomena exhibited by different voltaic combinations.* In this enunciation we certainly have the basis of almost all the electro-chemical theories and hypotheses which have elsewhere been' trumped up as original, and the experimental evidence upon which it is founded has been no less important, as suggesting the general mode of proceeding in all analogous inquiries.

But the discovery of Oersted has proved a new era, not merely in electro-magnetism, but in electro-chemistry, and has opened a tempting field of inquiry, in which Sir Humphry, among many others of note and eminence, has thought it right to try his strength. For detecting electric currents of feeble intensity, the author has used the electro-magnetical instruments which have been called Cumming's *galvanometer*, and Schweigger's *multiplier;* a delicate condensing electrometer was also employed. He proceeds, first, to the consideration of the electrical and chemical effects exhibited by combinations containing single metals and one fluid; and some of the experiments are both original and curious. We shall take the first as an instance. "When two pieces of the same polished copper connected with the platinum wires of the multiplier were introduced at the same time into the same solution of hydrosulphuret of potassa, there was no action; but if they were introduced in succession, there was a distinct, and often, if the interval of time was considerable, a violent electrical effect: the piece of metal first plunged in being negative, and the other positive. This result depends upon the circumstance of the production of a new combination, which is negative with respect to the metal; for after the formation of the sulphuret of copper, the plate of copper that has first been plunged into the solution exhibits the same negative state with respect to polished copper, whether introduced into saline solutions, or alcaline, or acid menstrua. The electrical effect, therefore, does not depend on so simple a condition as would at first appear, and it may be in fact referred to the combinations containing two metallic substances and one fluid. The grey sulphuret of copper is negative in solutions of hydrosulphuret to clean copper; and the superficial coating has apparently similar electrical powers to this substance."

In the same way protoxide of copper is shown to be negative to metallic copper and to the sulphuret; and, indeed, the production of electrical currents occurs generally whenever new products adhering to the metallic surfaces are formed. The chemical changes produced in the fluid by the ternary combinations thus formed, are always such as tend to restore the disturbed equilibrium—hydrogen and sulphuretted hydrogen passing to the negative side, and oxygen to the positive; so that the oxides are revived.

The author next considers electrical combinations, consisting of two imperfect and one perfect conductor; or two fluids and a metal, or charcoal; and controverts Becquerel's notion, that electricity is developed on the *combination* of acids and alcalies, plausibly referring the phenomena to the contact of metals, change of temperature, evaporation, and other analogous causes.

When platinum is brought into contact with an acid, the pole touching the acid is negative, the other positive; and the reverse is the case when it touches an alkali. Rhodium, iridium, and gold, act like platinum. With silver and palladium the action is greater, it being generally exalted in proportion as the chemical attractions are stronger.

In combinations of two conductors of the more perfect class, and one fluid, the order in which the metals exhibit electricities depends upon their oxidability; the most oxidable metal being positive in respect to those below it. This section contains several interesting conclusions deduced from experiments which scarcely admit of abridgment, and which our limits forbid us transcribing at length.

The seventh section treats of the accumulation of electricity, and the chemical changes it occasions in voltaic arrangements. Here we think some of Sir Humphry's conclusions are rather premature, at least, if intended to be applied *generally*. The cases which he brings before us are well and clearly detailed; but we should like to have had his opinion upon certain other instances of electrical excitation, more especially of those exhibited in some of the modifications of De Luc's column.

The paper concludes with some "General observations and practical applications." The former are chiefly theoretical and unimportant; the latter relate to the inefficiency of the protector for the copper sheathing of ships, to the preservation of iron boilers, and of the paddles of steam-vessels.

XIV. *On the discordance between the Suns observed and computed right Ascensions, as determined at the Blackman-street Observatory in the years* 1821 *and* 1822, *with Experiments to show that they did not originate in instrumental derangement. Also a Description of the Seven-feet Transit with which the Observations were*

procured, and upon which the experiments were made. By James South, Esq., F.R.S.

The contents and objects of this communication are sufficiently enunciated in its title; its voluminous details do not admit of abridgment.

xv. *On the Existence of a Limit to Vaporization. By* M. Faraday, Esq., F.R.S., &c.

Several facts and some well-imagined experiments are brought to bear affirmatively upon the above question.

xvi. *On Electric and Magnetic Rotations. By* Charles Babbage, Esq., F.R.S., &c.

In this long but interesting paper, a variety of new electro-magnetic facts are expounded, and it contains distinct and clear details of a numerous series of experiments in illustration of various phenomena of electro-magnetic excitation; many of these are new, and all of them well conceived, but as we shortly hope to lay before our readers a general historical view of the whole subject, we shall defer for the present an abstract of Mr. Babbage's labours in this department of physical science.

xvii. *Case of a Lady born Blind, who received Sight at an advanced age, by the formation of an Artificial Pupil. By* James Wardrop, Esq., F.R.S., &c.

This lady had reached her forty-sixth year when placed under Mr. Wardrop's care. He details the particulars of the operation, and of her gradually-acquired habit of distinguishing external objects. Forty-two days after the operation, she had acquired a pretty accurate notion of colour, but not of distance, nor of forms; neither could she without much difficulty direct her eye to an object; so that when she attempted to look at any thing, she turned her head in various directions, until her eye caught the object of which it was in search. She still entertained, however, the same hope which she expressed soon after the operation, that, when she got home, her knowledge of external things would be more accurate and intelligible, and that when she came to *look* at those objects which had been so long familiar to her *touch*, the confusion, which the multiplicity of external objects now caused, would in a great measure subside.

xviii. *On the Progressive Compression of Water by High Degrees of Force, with some Trials of its Effects on other Fluids. By* I. Perkins, Esq.

A column of water eight inches long, subjected to a pressure of

2000 atmospheres, is here stated to suffer a compression equal to one-twelfth of its length. Concentrated acetic acid, compressed with a force of 1100 atmospheres, was found to be beautifully crystallized, and this Mr. Perkins calls " a most remarkable result," not being apparently aware that the said acid is very prone, under common circumstances, to crystallize at temperatures below 40°.

We should call Mr. Perkins's most remarkable result his liquefaction of atmospheric air, which, he says, under a pressure of 1200 atmospheres was seen upon the surface of the quicksilver in the form of " a beautiful transparent liquid, in quantity about one-two-thousandth part of the column of air." Not a word more is said upon this singular result, nor is any thing new concerning the condensation of gases, generally, brought forward.

xix. *On the Figure of the Earth. By* George Biddell Airy, M.A., &c.

Mr. Airy's name is sufficient warrant for the value of this paper, which consists in mathematical discussions, not of a nature, in their details or results, to interest the general reader.

II. *Transactions of the Horticultural Society of London.* Vol. VI. Part the Fourth. London, 1826.

An opportunity of an early perusal of this work enables us to give a brief abstract of its principal contents before it can have found its way into general circulation. We may premise that it is illustrated by two coloured plates, the beautiful execution of which reflects much credit upon the artists employed.

i. *Method of cultivating the North-American and other hardy Orchideous Plants. By* Mr. Stewart Murray, *Curator of the Botanic Garden at Glasgow.*

Mr. Murray cultivates these plants perhaps with greater success than any other individual in England. He makes use of a compost of " one-third leaf mould, one-third turfy peat recently taken from the moors, and partly full of the roots and stems of common heath," and one-third sphagnum and sand, in equal proportions. With these materials he forms a border, over which he fixes a common glazed frame, under which the orchises are planted, " care being taken to keep the surface a little higher for those kinds which require less moisture, such as Cypripedium arietinum, and others." In the winter this frame is covered with mats, and in the summer it is shaded in very sunny weather.

ii. *Plan for obtaining a Second Crop of Melons. By* Mr. Charles Harrison, *Gardener to Stuart Wortley, Esq.*

When the first crop of fruit is nearly gathered, cuttings are taken from the plants and struck in 24 pots. The cuttings are selected from the ends of bearing shoots, and when rooted are placed in the same bed as that in which the mother-plants grew, but re-prepared for the purpose. The new plants grow rapidly, and produce fruit in three weeks after being placed in the renewed beds, or in a month from the time when they were first separated from their nearly-exhausted parents.

We have reason to know that this is an excellent method, and deserving of universal adoption for a late crop. It is obvious that it effects a considerable saving of time at a season when it can be least conveniently spared; it must also be considered an ingenious application to melons of the common practice of substituting viviparous for oviparous propagation, so advantageously and universally employed in arborescent fruits.

iii. *On the Cultivation of Celeriac as practised in Denmark and Germany. By* Mr. Jens Peter Petersen.

This delicious esculent is now becoming so common at our tables, that it is a matter of some importance to the public to be made acquainted with the most successful method of cultivating it; and the more especially because its excellence depends in a high degree upon the perfect manner in which it is grown. The author, himself a native of Denmark, where the management of this root is very skilfully practised, recommends "a light, moist, and well-manured or rich soil;" the dung to be used must be in a perfect state of decomposition. The seed, for a summer or autumn crop, is to be sown in a hot-bed in February; for a winter crop, at the latter end of March or thereabouts. The young plants are to be inured to the open air by degrees, and, when strong enough, are to be planted in rows about a foot apart. When they have acquired about half their growth, part of the mould is to be removed from about their roots, and all the side roots are to be carefully cut off, the mould being replaced as soon as the operation is performed, and the plants refreshed by a plentiful watering. "In Denmark and the northern parts of Germany, the roots are generally taken out of the ground at the end of October, and preserved for winter use in sand, in a dry house, or in a pit made in the open ground, secured from frost."

iv. *On the Cultivation of the Nelumbium Speciosum. By* Mr. Alexander Stewart.

We need not tell our readers that this fine plant is the Cyamus or Holy Bean of the ancients, and that it is one of the most precious inhabitants of our stoves. In its native country it is the ornament of every stagnant pool, and even the stinking ditches around the walls of Pekin assume the appearance of the gayest meadows from the abundance and beauty of its blossoms. With us it unfortunately languishes, and is better known for the broad glossy green of its leaves than for its chiefest ornament, its flowers, which are rarely seen. It is usually cultivated in tubs of standing water placed in a stove; Mr. Stewart pursued that plan, and his plants were like his neighbour's. In process of time his tubs became leaky, and the water not only ran out, but could not be kept in. Nevertheless his plants throve the better. The unconquerable leaks at last overcoming the gardener's perseverance, the plants and the tubs were left to arrange the affair between them. The leaks came off conquerors, the water was effectually drained away, but the plants, instead of dying, as would have been predicted, grew the more luxuriantly, and at length threw up fine flowering stems. Whence Mr. Stewart concludes, that the secret in the management of the Nelumbium is to drain these plants during the growing season; and, we doubt not, most justly. The fact is, that, whether the Nelumbium is found in the rice-grounds of Egypt, the ditches of China, or the swamps on the borders of the Caspian, it always grows in places which are only occasionally and unequally inundated. Mr. Stewart has, therefore, discovered, by accident, what nature has long been in vain proclaiming to the gardening world, whose ears are too often sealed against her voice.

v. *Description of a Pit for Winter and early Spring Forcing. By* Mr. Alexander Stewart.

This is a sunk pit, covered with reeds instead of glazed sashes. The vegetables to be forced are planted in it about the month of October, and are arranged in such a way as to allow as free a circulation of air among them as can be obtained. They are carefully protected by their covers from the effects of frost, and are ventilated as frequently and effectually as circumstances will permit. When the winter-stock, with which this pit has been filled, is removed, it is filled with radishes, lettuces, early peas, potatoes, and other early spring vegetables, " all of which can be got at least three weeks earlier in the pit than in the open borders."

vi. *On the Effects produced on Vegetables by the Combination of Heat and Moisture at different Periods of the Year. By* Mr. Archibald Gorrie.

Mr. Gorrie thinks that many useful and philosophical hints upon this subject will be found in a paper by John Frederick Daniell, Esq., published in a previous part of the Transactions, and that, in the same paper, many of the successful modes of operation are satisfactorily accounted for, which untutored sagacity had stumbled on, and which experience has sanctioned. Mr. Gorrie also thinks, that no observations have hitherto been made "at all calculated to elucidate an inquiry so generally interesting," as that of the degree of heat which is requisite "to bring certain plants into flower, or to mature their fruits or seeds in the open air, and in a given time, or the effect which a greater or less supply of moisture at different periods has in accelerating or retarding the progress of vegetation." To this subject, therefore, (Mr. Daniell having taken possession of the rest of the field of atmospheric investigation,) the author has applied himself, and the consequence is, a table showing the fluctuations of the rain-gauge and the thermometer in the Carse of Gowrie in 1823-4-5. For this, we must refer to the work itself, and content ourselves with stating, that the result of Mr. Gorrie's calculations, according to the twenty-one decades of Professor Playfair's formula, is, that the greater the fall of rain in a given season, the colder that season will be found to be.

vii. *On the Cultivation of Plants in Moss.* *By* Mr. John Street.

Mr. Street has ascertained, that many plants thrive better if planted in common moss than in garden mould. The mosses he uses are various species of hypnum, collected with the decaying stalks and leaves which are found amongst them. "The mosses so collected are pressed closely into the pot, and the plants are put into them as if into mould." This is quite a new, and certainly a remarkably neat method of managing many plants. For a *jardin fenêtrière*, it is better than any mode which can be devised; as our own observation enables us to say that plants treated in the way Mr. Street describes succeed perfectly; in some cases, indeed, far better than under ordinary management.

viii. *Description of a Pit and Stoves heated by Fire and Steam jointly.*
By Mr. William M'Murtie.

"The outline of the construction of these houses is this: instead of a deep pit to be filled with tan, a shallow one is formed by turning a flat brick arch over the space commonly occupied by the tan bed. Into and through the hollow chamber thus formed, a flue constructed in the usual manner is carried. On the top of the fireplace which heats this flue a boiler is fixed, which is supplied with water by a ball-cock; the lid or cover to the boiler has a pipe from its top, which is carried into the chamber above the flue, and the steam, which is produced by the water when boiling, is thus conveyed into and fills the entire chamber. From holes which are made in the sides of the pit at about four feet, or the width of each light, apart from each other, both in the back and front of the chamber, the steam and heat from the chamber are admitted into the house at pleasure, and when this is not desired the steam escapes by another opening out of the chamber into the external air."

Such is the construction of the pit, which the author has used with the greatest success at Lord Anson's, for upwards of twelve years. The principle is applicable to pineries, vineries, and stoves of all descriptions, into which a tan-pit is admissible. As the writer of this paper is a gardener of first-rate reputation, his observations may be safely recommended to serious consideration.

ix. *Account of some New Seedling Pears. By* Thomas Andrew Knight, Esq., F.R.S., &c. &c., President.

Mr. Knight has long been attempting "to create new varieties of dessert pears which would prove capable of being grown abundantly upon standard trees, in all moderately favourable situations, by fertilising the blossoms of the swans'-egg and other hardy varieties with the pollen of the late winter varieties of French pears." Out of many hundred seedling plants thus raised, about forty have borne fruit. "Most of these were of little value; a part not having acquired maturity or perfection, owing to defect of climate; some were deficient in flavour, and others had a disposition to decay internally." Six of the last varieties are described, which Mr. Knight expects will become important acquisitions. In this paper we meet with an observation which we believe has not been made before, that the first blossoms of all seedling trees are uniformly less capable of bearing unfavourable weather than those subsequently produced by the same trees. This should be borne in mind by the experimental horticulturist, as the knowledge of the fact may frequently prevent a new variety being condemned for the faults of its youth, which its old age would probably correct.

x. *On Blacking Garden Walls. By* Mr. Charles Harrison.

The writer finds that this operation is very useful in maturing the buds upon fruit bearing shoots; in which we do not doubt that he is perfectly right; but beyond this we suspect that no advantage is gained. Certainly the maturity of the fruit is in no degree accelerated by the practice, as has been erroneously suspected.

xi. *Account of a Plan for Forcing Grapes in Borders under Glass. By* the Rev. Blakley Cooper.

Mr. Cooper very justly objects to the common method of forcing vines, by applying the stimulus of heat to their branches within a stove, at a season when their stems and roots on the outside of the house are subject to the influence of a very different, and frequently unfavourable temperature. He avoids this inconvenience by planting his vines in the inside of the stove, and forcing their roots to grow in the inside also. For the details of his manner of managing this, we must refer to the paper itself.

xii. *On Glycine Sinensis. By* Joseph Sabine, Esq., F.R.S., Secretary.

xiii. *On the Pæonia Moutan, or Tree Pæony, and its Varieties. By* the same.

These papers are incapable of abridgment. They contain an elaborate history of the plants described, and of their mode of cultivation. Several new varieties of the tree pæony are described, one of which, raised from seed by the Earl of Mountnorris, after whom it is named, is illustrated by a splendid coloured figure.

xiv. *Report upon the effect produced on certain Plants in the Garden of the Horticultural Society, by the Frost which occurred during the night of April 29th,* 1826. *By* John Lindley, F.L.S., &c. &c., Assistant Secretary for the Garden.

This paper is confined to observations upon the effects of one of those severe spring frosts, which are so frequently destructive of all the hopes of the cultivator in the climate of this kingdom. That which was the immediate cause of the report, was one of the most fatal to vegetation which has ever been experienced. We have not space for the remarks which are made upon its effects in the garden of the society; they appear to have been extremely unequal, and not to have taken place in a degree corresponding with the latitudes in which the plants are naturally produced; for example: "the same cold which entirely destroyed the blossoms of Azalea viscosa, and of the other American Azaleas, scarcely affected Azalea pontica; and yet the geographical range of the former ones reaches as high as 50° N. latitude on the mountains of Canada, while that of the latter does not advance farther than 40° north in the lower regions of the precipices of the Caucasus."

xv. *On the Propagation of Zamias. By* Mr. Francis Faldermann.

The writer having found a Zamia rotten at the heart, expected to save it from further decay, by filling the cavity with sand. The consequence of this operation was, that every scale with which the stem is clothed shot forth roots and leaves, and produced a young plant. The curious tribe of plants to which Zamia belongs is therefore to be propagated by the scales.

This fact is of course to be explained, by the presence in the axillæ of the scales of latent gemmæ, which are called into action by the particular condition in which the plant is placed.

xvi. *Description of Stoves for the Growth of Melons and Cucumbers. By* Mr. John Haythorn, Gardener to Lord Middleton.

These may rather be termed heated pits, as they are of very narrow dimensions, and much sunk below the ground-level. They are heated by flues that communicate with cavities, the air of which is warmed, and then finds its way into the house. Water is conveyed by means of pipes to the top of the flues, whence it escapes as steam into the interior. A border is carried over the flues, a cavity being left between them. The melon and cucumber plants are trained to wires attached to the rafters. This method is said to give the gardener a great command of regulated humidity, an advantage which is undoubtedly of the greatest importance.

xvii. *Notice of certain Vineries at various places in Scotland, with Arched Hanging Trellises. By* Mr. William Smith.

This paper is for the purpose of recommending a mode of training the vine, by which the inconvenience experienced from the interception of light by the foliage of the plant is avoided. Instead of carrying the branches of the vine over the glass, as is commonly practised, and therefore parallel with the roof, a framework of treillage is fixed to each rafter, at right angles with the roof, and upon this the training is arranged. The writer says, "The hanging trellis has this superiority, that while it admits light to the interior of the house, it increases the space for training considerably." Several varieties of this method are illustrated by engravings.

xviii. *Observations on the Qualities of Newly-raised Fruits, exemplified in Plums. By* Thomas Andrew Knight, Esq., F.R.S., &c., President.

Mr. Knight has remarked, in the course of his very extensive experience in the line of experiment alluded to, that the prevalent defect of seedling fruits is, the want of a sufficient quantity of saccharine matter. The pulp is often of very good quality, and the juice is frequently abundant, but it is generally found to be in a large majority of cases insipid and watery. Mr. Knight has endeavoured to correct this defect, by raising plants from seeds of wild and acid varieties, which had been impregnated by highly-cultivated varieties. The six plums which are the subject of this communication have been so obtained: from a coloured plate which accompanies them, they seem to be very handsome, and they are stated to be wholly destitute of the bad qualities which it was the object of Mr. Knight's experiment to avoid.

xix. *On the Application of Tobacco-water in the Destruction of Insects.* *By* Mr. Joseph Harrison.

In the process of preparing tobacco for use, a liquid is finally expressed from it, which is very cheap, and highly destructive of animal life. This, mixed with three to five parts of water, is found by the writer an effectual remedy for the aphis, caterpillars, and similar insects which infest fruit trees.

III. *Remarks on Mr.* PEYRON'S *Account of* EGYPTIAN PAPYRUS. Mem. di Torino, xxxi. 1826. *In a letter to Count* POLLON.

My dear Sir,

Considering the readiness which you can bear witness that I have always shown, to comply with any wish of your countrymen, to obtain information respecting the progress of literature and of science in this country, I confess that I did expect to have been generally treated by them with indulgence rather than with severity; and I am quite sure that you were as fully persuaded as myself, that they would have justified this expectation. I am not, however, about to enter a protest with the public on the occasion; I wish only to explain a few particular circumstances which appear to have been mistaken; and to make some admissions which my friends may think due from me to my opponents: for opponents, it seems, I have; and I cannot help feeling that the learned and ingenious Mr. Peyron is one of them; at least, such is the unavoidable impression that is produced by the *manner* in which he has pointed out several imperfections, in my attempts to illustrate the antiquities of Egypt, which *further documents* only have enabled him and others to correct.

In his essay, read the 27th May 1824, he says of my attempt to obtain a cast of Drovetti's pillar, then lying unnoticed at Leghorn, "sebbene allegasse i diritti del suo sapere, dicendo *essere lui il solo uomo vivente, che potesse compiutamente apprezzare il valore di tal monumento.*" Now this passage, printed as it is in italics, has the manifest tendency to accuse me of a degree of *presumption* which its author thought unwarrantable; and there is no doubt that the majority of his readers will have been easily influenced by such an insinuation.

I must however observe, that as a perfect stranger, writing, as I did, without introduction, to a merchant, not conversant with literature or with literary men, it was necessary to say something that should make my correspondence worthy of his notice; and with regard to the truth of the fact, it would have been a little more candid if Mr. Peyron had stated his own belief, whether I *was* or *was not* at that time "the only person living that could fully appreciate the value of the object in question."

Akerblad was dead: Champollion had not *then* done any thing worth mentioning on the subject of hieroglyphics. I had published, *seven years* before, and had sent directly to Champollion a literal translation of the two Egyptian inscriptions on the pillar of Rosetta: I had sent the name of Ptolemy and the interpretation of many other characters to the English travellers in Egypt: it was *after* my return from Italy that Champollion received the name of Cleopatra, as ascertained in Egypt by Mr. Bankes from my letters; it is hence that *he himself* dates the origin of his system: *Ergo opera illius mea sunt!* And I willingly add, of this new Achilles, *fortemque in fortia misi!*

However this may be, it was late in 1822, that Champollion published his Letter to Mr. Dacier; and I shall be very much surprised if Mr. Peyron can produce a single page in any work that I could have seen in 1821, which could have convinced me that I was mistaken in the grounds of my *presumption:* nor do I feel that I ought to be ashamed of the impression, or of the purpose to which I attempted to apply it.

The most serious *error* that has been directly laid to my charge, in Italy and in Germany, is the introduction of the name of Isis, instead of Thebes, where there was a defect in Mr. Grey's manuscript, at present in the British Museum. On this Mr. Peyron observes, "*Lacunam vix decem litterarum ita supplebat:* των δουλων Ισιδος της μεγαλης," which, if it means any thing by the *decem litterarum*, means that I had inserted twenty four letters instead of ten: for he says, in the next page, "mallem tamen τῶν ἀπὸ Διοσπόλεως," as if he thought that eleven letters might safely be admitted; and it is obvious that we cannot estimate the size of the letters in such manuscripts with any thing like accuracy. It is easy to count the letters in the passage as he has quoted it; and I have inserted *exactly ten* letters instead of eight; and for these ten there would have been ample room. I naturally thought of Isis as preceding "the great" and following the first letter of "servants," from the frequent occurrence of such epithets in the Greek inscriptions still extant in Egypt; and having made up my mind that it was a happy conjecture, it never occurred to me to discard it, as I might otherwise have done, when I found "Diospolis the great" so frequently mentioned in other passages. The second letter of the first word was but imperfectly effaced, and it *might* have led me earlier to the true reading: but the great manuscript of Turin, now published by Mr. Peyron, leaves no doubt on the question; and with this point ascertained, it was easy for Mr. Champollion to make out the name of Diospolis, "the splendid temple of Amun," in the contracts of Mr. Grey, as well as in that of Casati, and to find the numbers of the contracting parties preceding it. I had already observed my mistake of son for daughter, in the epithet of Lubais and Tbæais, before I received Champollion's translation of the same passages (p. 132), which is of course copied in great measure from mine, but with the advantage of documents more lately discovered.

Mr. Peyron remarks, with an air of triumph, " *jam corruit quidquid Cl. Anglus de Hierostolistis templa aut simulacra deorum ornantibus* COMMENTUS *fuerat:*" as he had before observed, " *Così i Colchiti non sono più* DRESSERS, *sacri paratori delle Divinità, come sospettò il Y.*" He confesses, however, that " *Etymon tamen acute vidit;*" and adds, that " *dicti sunt ab* INVOLVENDO *cadavere;*" as I had said that " Cholchytes may possibly be a derivative of DCHOLH or JOLH, to *dress*, to *put on.*"

I had lately been looking over the Enchorial deeds of Mr. Grey, with the hopes of advancing a few steps in the study of the language. I had observed the agreement of the numbers in the registries and in the Egyptian text; I had found the " talent" of the German critics expressed by an Egyptian symbol nearly resembling that of the Greek lines; and I had ascertained that the Enchorial character, for what is called a cubit in this manuscript of Turin, contains a part like the " Arura" of the inscription of Rosetta. It will be obvious, from a brief abstract of this invaluable document, how much may hereafter be expected from a patient and repeated study of the Enchorial manuscripts with its assistance.

The record begins with a date: " In the year LIV, the 22nd of Athyr, at Diospolis. Before Heraclides, one of the captains of the body guard, the commander of the Perithebaic Nome, and chief officer of the customs: in the presence also of Polemo, a captain of the body guard, Heraclides of the same rank, and Gymnasiarch, Apollonius, the son of Apollonius, and Hermogenes, as his friends, Pancrates, a lieutenant, Comanus, a general, Paniscus, son of Ammonius, a colonist, and several others.

" There appeared Hermias the son of Ptolemy, of the Ombitic Nome, as a plaintiff against the Cholchytæ of the place, Horus, and Psenchonsis, and Chonopres, and their family."

" A memorial was read, which had been addressed to Hermias, Strategus, or commander in chief and governor of the Nome," in which the plaintiff Hermias accuses the Cholchytæ, and mentions a former memorial addressed to Demetrius, the Epistrategus, or high commander, in the preceding year. The latter memorial had been referred by Hermias the Strategus to the judgment of Heraclides. The cause having been pleaded by Philocles for Hermias, and by Dinon for the Cholchytæ, Heraclides sums up the evidence, discusses the arguments, and gives sentence for the defendants.

The claim of Hermias seems to have been founded on an assertion of property only, supported by an admission of Lobais or Lubais, who was one of the vendors of the house, that she neither had nor had ever had any title to the property in question.

He had asserted in his memorials (p. 1,) that his ancestors had held possessions in Diospolis throughout their lives; and that the defendants, whose proper habitation was confined to the Memnonia, where their ancestors had resided, knowing that he had been induced by the troubles of the times to change his residence had usurped his house, which was situated in the south-western part of Diospolis, to the north of the course leading to the canal of the great goddess Juno, to the south of the course leading to the temple of Ceres, the walls of which were left standing (p. 2), and when they had repaired it they continued to occupy it . . . . And not content with inhabiting it, they had received dead bodies in it, not considering the penalties to which they became liable, especially as they bordered on the courses or parades of the great goddesses Juno and Ceres, which would be polluted by dead bodies, and by those who have the charge of them.

The Cholchytæ allege that Hermias had already been nonsuited in a similar action on the same grounds: that they had been in possession of the house for many generations, and they produced their Egyptian title deeds translated into Greek. One of these was dated in the month Pachon, the XXVIIIth of Philometor, in which "Teephibis, the father of Psenchonsis, one of the defendants, and of Chonopres, bought of Elecis and Lobais, and Tbæais, and Senerieus, and Erieus, and Senosorphibis, and Sisois, otherwise called Erieus, in all seven vendors, seven and a half œcopedic cubits, or house cubits, of the southern part of ten "cubits" of unproductive ground which they possessed. By a second deed, Asos, the father of the defendants Nechutes and Asos, and a *younger Nechutes*, and their sister Nechuthis, bought of the same persons two cubits and a half, in the same year and month. A third deed, of the year XXXV, in the month Mesore, of the same Philometor, attests the sale of a fourth part of the same house, being three cubits and one third, to Pechytes, the father of the others, Panas, and Patus, and Pasemis, and Arpchemis, and Senamunis, by Ammonius and Zbendetis, to whom it belonged. The duties on these transactions had also been paid into the Office for the purchase of the tribute; and the parties had kept undisturbed possession of the premises for thirty seven years. Some passages of the "benevolent edict" were also alleged, in which it was decreed, that such persons as were in possession of property, without being able to produce their title deeds, should still be allowed to hold it.

It was added by Dinon, that since the plaintiff's father had left Diospolis at the time of the troubles under Epiphanes, that event must have happened eighty eight years ago, a period too remote to allow of the present controversy. For adding to the twenty four years of Epiphanes thirty five of Philometor, and twenty nine of Evergetes from twenty five to fifty three inclusive, we have eighty eight years (see Discoveries in Hier. Lit. p. 143). That he produced no document in support of his claim, except a fraudulent sentence obtained by collusion with Lobais, who had no interest in defending the action. That, with respect to the removal of the embalmers to the Memnonia, the subject was wholly foreign to the question, for that Horus and his colleagues are not embalmers, but dressers; their business being also on popular festivals, and on other sacred days, to bring ashes or sand, and to strew with them both the course of Ammon and the temple, and to do the same in the temple of Juno: besides, in the annual processions, when Ammon passes over to the Memnonia, it is their business to be the leaders of the ceremony, they act as Cholchytæ, and are rewarded accordingly.

It is obvious, from the contents of this abstract, that many important points, in the Enchorial manuscript of Mr. Grey, are capable of further illustration from the record. For, in fact, all the *three* deeds of Mr. Grey are the actual title-deeds mentioned in this manuscript, though Mr. Peyron has identified but *two* of them: the purchasers mentioned in the registry of the third being Ammonius and Psenamunis, instead of Ammonius and Zbendetis or Zthenaëtis; but it seems probable that Zbendetis or Zthenaëtis had two names, as well as Sisois or Spois, who is also called Erieus; for the date of the deed agrees perfectly, as well as all the rest of the names and the measures.

Thus, then, this singular train of fortunate coincidences in the revival of Egyptian literature is still marvellously continued. The first Greek manuscript of Mr. Grey, "The Antigraph of an Egyptian deed," arrived, as if by magic, while the copy of the Egyptian original of Casati, which I had just been studying, was still lying on my table; and when, for the few last weeks, I had been accidentally led to resume the consideration of the Egyptian deeds of Mr. Grey, the record came from Turin to my assistance, and offered me a precise confirmation of the date which I had before assigned to the first deed, from the Egyptian text, as the more probable of two possible alternatives, and afforded me an exact description of the very spots of ground to which the three conveyances relate.

I have only to express a hope that Mr. Peyron will not omit to give the public a fac simile, if not of the whole record, which he says is written too fairly to present any difficulties, yet at least of the names which appear to differ from the most natural reading of Mr. Grey's manuscripts: for instance, Sisois for Spois, Teephibis for Teephbis, and Zbendetis for Zthenaëtis. It is true, that his readings appear in general to agree best with the Egyptian writing, but it would be satisfactory that, wherever there can be a doubt, the whole evidence should be laid before the public.

Park Square, 20 *Dec*. 1826. * * * *

ART. XIII. ASTRONOMICAL AND NAUTICAL COLLECTIONS.

No. XXVIII.

i. *Miscellaneous* CORRECTIONS.

1. IN the titles of the last three numbers of these Collections, for XXII, XXIII, and XXIV, read XXV, XXVI, and XXVII.

2. In the last number, first paper, the length of the pendulum on the level of the sea should stand thus:

Paris	1.00002271
Rio Janeiro	.99783538
Cape of Good Hope	.99871582
Isle of France	.99794215
Isle Rawak	.99709575
Isle Guam	.99759331
Isle Mowi	.99792816
Port Jackson	.99877424
Falkland Islands	1.00022319; Freycinet, p. 26.

3. In the XXIInd number of the Astronomical and Nautical Collections, Mr. Henderson has pointed out errors in the places of μ♊, κ♍, and 1β♏, as laid down in the IIIrd number; Professor Schumacher had before noticed two other errors:

N. 70 A. Oph. for 24″,5 read 34″,5
79 σ ♐ — 21,5 — 31,5

The error of the place of 1β♏ still requires correction in the Nautical Almanac, and in the Greenwich Observations for 1825, Part I. Mr. Henderson finds the seconds, reduced to 1825, 16.53 and 16.84, from two observations made at Greenwich, and 16.63 from the catalogues of Bradley and Piazzi, according to Bessel's formulas.

These errors render it improper to employ the Elements of Occultations for *these stars*, as computed in the Nautical Almanac, without proper precautions.

4. *Corrections* to be applied to some of the *Elements of Occultations*, published in the *Nautical Almanacs;* as suggested by Mr. **Henderson.**

Element to be corrected.	μ♊	1β♏	κ♍
	Sept. 1824 to Feb. 1826.	April, 1826 to End of 1827.	Jan. 1829 to End of 1829.
Conjunction in A. R. . .	add 25^s	add $1^m\ 10^s$	
Diff. dec. ☽ . . . N.	sub. 2″	sub. 5″	add 52″
Diff. dec. ☽ . . . S.	add 2	add 5	sub. 52
A. R.	add 1^s	add 3^s	
☽'s Declin.	sub. 2″	add 5″	
Near. app. diff. dec. N.	. . . .	sub. 5	add 50
Near. app. diff. dec. S.	. . . .	add 5	sub. 50
Time of nearest approach	. . . .	add $1^m\ 9^s$	add 27^s

These erroneous Elements were caused by *errors* in the *mean* places of the stars.

Page	
	N. A., 1826.
182,	O. A. of 1β♏, April 23, for 82° 20′, read 83° 20′.
	N. A., 1828.
194,	A. R. of 1β♏, . . . for $15^h\ 55^m\ 13^s,3$, read $15^h\ 55^m\ 16^s,3$.

5. Errors in Hutton's Tables of Logarithms, observed by Mr. N. **Grut**, since the publication of the XXIVth number of these Collections.

P. 188	N.	11230,		for 0503792,	read 0503798
193	Diff.	315	8	— 352	— 252
		311	2	— 22	— 62
194	N. 14049			— 0454	— 6454*
	Diff.	303	7	— 312	— 212
196		281	1	— 38	— 28
201		247	1	— 28	— 25
266	N.	5026	7	— $283\bar{0}$	— 2830

ii. *Occultations of Fixed Stars, not less than the Fifth Magnitude, in the year* 1827, *calculated for the Royal Observatory at Greenwich. By* Thomas Henderson, Esq.

Date.		Names of Stars.	Magnitude.	Immersion and Emersion. Mean Time.				Apparent difference of Declination.			Point of Moon's Limb.	
					H.	M.	S.	* ′	″		°	
Jan.	5	*π* Piscium	5	Imm.	10	31	25	1	53	N.	46	L.
				Em.	11	37	38	6	30	S.	153	R.
	14	*κ* Cancri	5	Imm.	9	28	28	9	43	S.	168	L.
				Em.	10	34	24	1	1	S.	62	R.
	19	*i* Virginis	4	Imm.	14	21	33	15	59	S.	156	R.
				Em.	14	53	9	9	48	S.	104	R.
	22	*ω* Ophiuchi	5	Imm.	19	4	32	4	20	S.	117	L.
				Em.	20	17	14	0	8	S.	91	R.
Feb.	10	1*a* Cancri	4	Imm.	12	9	27	0	50	S.	82	L.
				Em.	13	21	0	10	38	N.	71	R.
	15	*g* Virginis	5	Imm.	10	34	46	11	36	S.	173	L.
				Em.	11	31	9	0	0		58	R.
	18	1*ω* Scorpii	5	Imm.	14	34	41	10	11	S.	159	L.
				Em.	15	34	42	3	38	S.	80	R.
	20	1*μ* Sagittarii	4	Imm.	17	5	13	9	32	S.	152	L.
				Em.	18	1	13	9	54	S.	109	R.
		2*μ* Sagittarii	4	Imm.	17	49	56	9	39	N.	74	L.
				Em.	18	49	55	8	35	N.	46	R.
	21	*d* Sagittarii	4	Imm.	18	53	16	1	23	S.	115	L.
				Em.	20	3	51	6	40	S.	105	R.
March	14	*ψ* Virginis	5	Imm.	12	12	19	4	15	S.	117	L.
				Em.	13	23	43	9	33	N.	55	R.
	17	*λ* Libræ	4	Imm.	15	15	32	8	42	S.	129	L.
				Em.	16	24	43	2	58	S.	106	R.
April	23	*δ* Piscium	4	Imm.	16	48	23	13	50	N.	63	L.
				Em.	17	31	10	5	55	N.	28	R.
May	8	*ψ* Virginis	5	Imm.	7	53	23	5	38	S.	129	L.
				Em.	9	4	19	8	20	N.	53	R.
	13	2*μ* Sagittarii	4	Imm.	13	49	13	11	38	S.	143	L.
				Em.	14	36	31	13	25	S.	143	R.
July	2	*g* Virginis	5	Imm.	8	43	37	13	18	S.	123	L.
				Em.	9	40	12	4	24	S.	136	R.
	7	2*μ* Sagittarii	4	Imm.	10	56	47	8	34	S.	122	L.
				Em.	11	53	54	11	17	S.	140	R.
Aug.	25	*χ* Virginis	5	Imm.	6	51	42	14	37	S.	124	L.
				Em.	7	30	38	8	23	S.	160	R.
	26	*a* Virginis	1	Imm.	2	56	41	5	17	N.	71	L.
				Em.	3	52	34	14	27	N.	33	R.

Date.	Names of Stars.	Magnitude.	Immersion and Emersion Mean Time.				Apparent difference of Declination.			Point of Moon's Limb.
							*			
				H.	M.	S.	′	″		°
Aug. 29	ψ Ophiuchi	5	Imm.	8	24	0	5	11	N.	47 L.
			Em.	9	27	16	6	19	N.	98 R.
Sept. 7	ε Piscium	4	Imm.	12	6	7	1	17	N.	106 L.
			Em.	13	16	28	11	14	S.	128 R.
17	ω Leonis	5	Imm.		under		horizon.			
			Em.	15	13	51	2	54	N.	40 R.
25	2β Scorpii	4	Imm.	6	34	57	16	2	S.	148 L.
			Em.	6	47	54	15	34	S.	168 L.
	1β Scorpii	2	Imm.	6	37	59	16	8	S.	154 L.
Oct. 12	*k* Geminorum	5	Em.	6	44	13	15	55	S.	163 L.
			Imm.	14	18	49	14	31	S.	155 R.
			Em.	14	43	34	13	29	S.	118 R.
14	κ Cancri	5	Imm.	12	43	26	7	18	N.	99 L.
			Em.	13	27	6	12	0	N.	2 L.
Nov. 1	ε Piscium	4	Imm.	4	9	47	11	28	N.	81 L.
			Em.	5	0	47	2	3	N.	43 R.
8	λ Geminorum	5	Imm.	15	1	23	11	55	S.	158 L.
			Em.	16	4	54	7	35	S.	122 R.
28	ε Piscium	4	Imm.	12	2	38	8	19	S.	88 L.
			Em.	12	44	30	14	33	S.	162 L.
Dec. 8	ω Leonis	5	Imm.	15	59	57	5	3	S.	112 L.
			Em.	17	21	56	6	56	S.	79 R.

The fifth column shows the apparent difference of declination between the star and moon's centre at the immersion and emersion; the letters N. and S. denoting the star to be north or south from the moon's centre. The sixth or last column shows the point of the moon's limb where the immersion and emersion take place, reckoning from the vertex or highest point; the letters L. and R. signifying to the left hand or right hand of the observer.

The circumstances attending the occultations of 1 and 2β Scorpii on the 25th September, may differ considerably from those above set down, or it may even happen that there will be no occultation at all, owing to the errors in the computed declinations of the moon and stars. By the Tables, the moon will make a very near appulse to κ Aquarii (5th magnitude) on the 14th June. At 12 h. 46 m., mean time, her north limb will be six seconds distant from the star; twelve miles south from Greenwich, the moon will be seen to touch the star, and further south the star will be eclipsed. But the errors in declination will also have a considerable effect upon these phenomena; and it is probable that the star will be occulted at Greenwich. These occultations are deserving the attention of astronomers, as Cagnoli's method of ascertaining the figure of the earth may be put in practice upon them.

None of the planets will this year be occulted at Greenwich. On the 21st February, the moon will make an appulse to Venus. At 21 h. 44 m. mean time, they will be at their least distance, when the moon's north limb will be 1′ 33″ distant from Venus's south limb.

iii. *Elements for computing the Eclipses of the Sun, and Occultations of the Planets by the Moon, in the year* 1827.

By THOMAS HENDERSON, Esq.

	Conjunction in A.R. Apparent Time.	Diff. Dec.	Relative H. M.	Relative Orb. Aug.	A. R.	☽'s Declination.	Nearest Approach.	Time of Nearest Approach, Apparent Time.	The Sun, or Planet's Horary Motion in A.R. in time.	Horary Motion In Decli-nation.	diame-ter.	Hor. Par.
☽	D. H. M. S.	′ ″ ☽	′ ″	° ′	H. M. S.	° ′ ″	′ ″	D. H. M. S.	S.	″	″	″
♀ Feb.	21 21 9 43	35 56 N.	33 13	N. 79 51 E.	19 11 10	18 5 13 S.	35 22	21 20 58 16	+ 10	+ 2	14	14
☿ March	28 1 12 53	74 15 S.	31 58	N. 72 15 E.	1 8 5	9 38 56 N.	70 43	28 1 55 22	− 2	−10	5	13
♂ March	29 13 58 37	24 30 N.	28 49	N. 76 41 E.	2 22 58	14 50 19 N.	23 51	29 13 46 52	+ 7	+36	2	4
☉ April	25 15 40 2	52 27 N.	28 10	N. 76 7 E.	2 11 19	14 6 50 N.	50 55	25 15 13 14	+ 9	+49	955	9
♀ June	21 7 27 37	8 41 S.	26 41	N. 83 57 E.	3 57 43	18 54 1 N.	8 38	21 7 29 40	+ 12	+41	6	6
☉ Oct.	20 4 20 55	50 24 S.	32 43	S. 74 24 E.	13 38 19	11 4 20 S.	48 33	20 3 56 3	+ 9	−54	966	9
III. Sat. ♃ } Dec.	14 18 17 49	55 9 S.	34 44	S. 76 7 E.	14 18 23	13 35 53 S.	53 32	14 17 54 58	+ 2	−12	0	1
II. Sat. ♃ } Dec.	14 18 18 47	55 26 S.	35 0	S. 76 5 E.	14 18 25	13 36 2 S.	53 48	14 17 55 55	+ 1	− 7	0	1
I. Sat. ♃ } Dec.	14 18 19 43	55 17 S.	34 55	S. 75 6 E.	14 18 27	13 36 10 S.	53 39	14 17 56 55	+ 2	− 9	0	1
♃ Dec.	14 18 22 19	55 7 S.	34 52	S. 76 5 E.	14 18 33	13 36 32 S.	53 30	14 17 59 31	+ 2	− 9	17	1
IV. Sat. ♃ } Dec.	14 18 33 33	54 35 S.	34 54	S. 76 3 E.	14 18 59	13 38 8 S.	52 58	14 18 10 56	+ 2	− 7	0	1
☿ Dec.	16 13 29 42	3 45 N.	35 10	S. 84 11 E.	16 5 32	18 20 44 S.	3 44	16 13 30 21	+ 9	−40	3	9

The places of the sun and moon have been taken from the *Nautical Almanac ;* those of Mercury have been calculated from Lindenau's Tables; and those of the other planets have been taken from Schumacher's *Ephemeris.*

The sign + denotes the motion in A. R. to be progressive; the sign —, retrograde. The sign + denotes the motion in declination to be towards the north; the sign —, towards the south.

The present list of occultations (ii.) will generally be found to differ from that inserted in Schumacher's *Astronomische Nachrichten*, Nos. 92 and 93; the differences sometimes amounting to 12 minutes of time. As great care has been bestowed upon the calculations of the present list, while the other appears to give only approximations, it is presumed that more reliance may be placed upon the former, which, however, is applicable only to the principal stars. When it is considered that it is often of great importance for astronomers to be informed, as nearly as possible, of the time when a phenomenon is to happen, the publication of the present list may not be deemed superfluous.

iv. *A Table of Refractive* DENSITIES, *computed from* Dr. BREWSTER'S *Experiments.*

A FEW years ago Dr. Brewster published, in his *Treatise on Philosophical Instruments*, (1813,) the results of some very ingenious experiments on the refractive powers of various substances, placed in contact with the object glass of a microscope, and so inclosed as to constitute a concave lens, which lengthened the focal distance of the glass more or less, according to the greater or less refractive density of the substance. Dr. Brewster did not compute the precise value of the index of refraction from these experiments, for want, as it appears, of a correct measurement of the curvature of the surfaces concerned. It was, however, observed, soon after the publication of Dr. Brewster's volume, by the writer of an article in the *Quarterly Review*, (No. 21,) that there was no occasion for any other measurement than that which the results of the experiments themselves afforded; and that, neglecting the thickness of the lenses, the refractive densities might be obtained from those results, by means of a very simple formula.

In fact, if we call the focal distance of the object glass alone e, the observed focal distance being f, we obtain the focal distance, g, of the lenticular portion of the substance under examination, from the equation $\frac{1}{f} = \frac{1}{e} - \frac{1}{g}$; and $\frac{1}{g} = \frac{1}{e} - \frac{1}{f}$: but it is well known that the focal distance of any lens varies in the inverse proportion of $r - 1$, r being the index of refraction: so that $g = \frac{a}{r-1}$, and $\frac{1}{g} = \frac{r-1}{a} = \frac{1}{e} - \frac{1}{f}$; whence $r - 1 = \frac{a}{e} - \frac{a}{f}$, and $r = 1 + \frac{a}{e} - \frac{a}{f}$.

Now, in Dr. Brewster's first table, p. 263, we have $e = 1.425$, and, for water, $r = 1 + \frac{a}{e} - \frac{a}{f} = 1.336$, and $\frac{.336}{a} = \frac{1}{e} - \frac{1}{f} = \frac{1}{1.425} - \frac{1}{2.309} = .7017 - .4331 = .2686$, whence $a = 1.251$, and $\frac{a}{e} = .878$; consequently $r = 1.878 - \frac{1.251}{f}$. In the same manner we find, for the second table, p. 264, $r = 2.31 - \frac{1.31}{f}$.

From these formulas the numbers marked (B 1), (B 2) have been calculated: those, which are marked B 3, are copied from Dr. Brewster's own table of refractive powers, p. 283, obtained from direct observations of the effect of a prism. The other parts of this table are principally taken from Dr. Young's *Natural Philosophy*, vol. ii. p. 296. It must be observed that the numbers marked W belong in general to the extreme red rays: hence they are commonly smaller than Dr. Brewster's, which express the mean refraction of white light. Several substances have been omitted for want of accurate definitions to ascertain their identity.

General Table of REFRACTIVE POWERS.

A vacuum	1.0000000
Atmospheric air, B 30, Th. 47°, N. A.	1.0002825
Ice, W, Y.	1.310
B 3	1.307

Water, W, B 3	1.336
Vitreous humour, W	1.336
from a haddock, (B 1)	1.340
from a lamb, (B 1)	1.345
from a pigeon (B 1)	1.353
Saliva, (B 1)	1.339
Expectorated mucus (B 1)	1.339
Aqueous humour, from a haddock (B 1)	1.341
Cryolith, B 3	1.344
Jelly fish, medusa aequorea, B 3	1.345
Vinegar, (B 1)	1.347
Port wine, (B 1)	1.351
Human blood, (B 1)	1.354
Oil of box wood, (B 1)	1.356
Ether, W	1.358
(B 2)	1.374
White of a hen's egg, (B 1)	1.359
B 3	1.361
Albumen, W	1.36
Brandy, C. (B 1)	1.360
Rum, (B 1)	1.360
Oil of ambergris, B 3	1.368
(B 1)	1.379
Alcohol, W	1.37
C	1.371
B 3	1.374
(B 1), (B 2)	1.377
Distilled vinegar, H	1.372
Saturated solution of salt, C	1.375
Muriatic acid, B 3	1.376
(B 2)	1.395
Oil of wine, (B 1)	1.379
Cornea of a lamb, (B 1)	1.386
Crystalline of a lamb, outer coat, (B 1)	1.386
Pus, (B 1)	1.395
Nitrous acid, B 3	1.396
(B 2)	1.404
Juice of orange peel, (B 1)	1.403
Crystalline of a pigeon, (B 1)	1.406
of a haddock, outer coat, (B 1)	1.410
Nitric acid, sp. gr. 1.48, W	1.410
C	1.412
(B 2)	1.410
B 3	1.406
Hydret of soda, melted by heat, (B 2).	1.411
Hydrophosphoric acid, melted and hot, (B 2)	1.423
cold, (B 2)	1.441
(B 3)	1.442

Gluten of wheat, dried, (B 1)	1.426
Yolk of a hen's egg, fresh, (B 1)	1.428
Crystalline of a lamb, middle coat, (B 1)	1.428
Fluor spar, W	1.433
(B 3)	1.436
Oil of rue, B 3	1.433
(B 1)	1.449
Sulfuric acid, W	1.435
B 3	1.440
(B 1)	1.442
(B 2)	1.446
Crystalline of a lamb, central part, (B 1)	1.436
Phosphorous acid (B 1)	1.437
Crystalline of a haddock, outer part, (B 1)	1.439
Spermaceti, melted, W	1.446
(B 1)	1.454
Crystalline of an ox, W	1.447 to 1.380
Oil of wax, C	1.452
Oil of wormwood, B 3	1.453
Bees' wax, melted, (B 1)	1.453
Alum, W	1.457
(B 1)	1.488
Oil of chamomile, B 3	1.457
(B 1)	1.476
Oil of lavender, B 3	1.457
(B 1)	1.475
Tallow, melted, W	1.460
(B 1), p. 260	1.466
p. 258	1.483
White wax, melted, (B 1)	1.462
cooled, (B 1)	1.507
Oil of poppy, B 3	1.463
(B 2)	1.483
Borax, C	1.467
(B 3)	1.475
Oil of lavender, W	1.467
(B 3)	1.457
(B 1)	1.475
Oil of peppermint, W	1.468
(B 1)	1.473
Oil of rosemary, B 3	1.469
(B 1)	1.472
Oil of olives, W	1.469
B 3	1.470
(B 1)	1.476
Oil of almonds, W.	1.470
(B 1)	1.481
(B 3)	1.483

Oil of brick, distilled from spermaceti oil, (B 1)	1.470
B 3	1.471
Oil of turpentine, rectified, W	1.470
common, W	1.476
B 3	1.475
(B 1)	1.476
(B 2)	1.485
C	1.482
Spermaceti oil, B 3	1.470
(B 1)	1.473
Oil of bergamot, B 3	1.471
(B 1)	1.473
Rape oil, B 3, (B 1)	1.475
Palm oil, (B 1)	1.475
Naphtha, (B 1)	1.475
Essence of lemon, W	1.476
B 3	1.481
(B 1)	1.489
Oil of dill seed, B 3	1.477
(B 1)	1.487
Oil of thyme, B 3	1.477
(B 1)	1.486
Oil of cajeput, (B 1)	1.478
B 3	1.483
Naples soap, (B 1)	1.479
Butter, cold, W	1.480
(B 1)	1.474
Oil of spearmint, B 3	1.481
(B 1)	1.496
Oil of mace, melted, (B 1)	1.481
cooled, (B 1)	1.525
Oil of pennyroyal, B 3	1.482
(B 1)	1.489
Oil of savine, B 3	1.482
(B 1)	1.485
Oil of juniper, (B 1)	1.482 to 1.491
B 3	1.473
Train oil, (B 1)	1.483
Linseed oil, W	1.485
(B 1)	1.487
Oil of wormwood, (B 1)	1.485 to 1.489
Castor oil, (B 1)	1.485
B 3	1.490
Florence oil, (B 1)	1.485
Camphor, W	1.487
C	1.500
(B 1)	1.496

Oil of fenugreek, B 3	1.487
(B 2)	1.488
Oil of hyssop, B 3	1.487
Windsor soap, (B 1)	1.487
Obsidian, B 3	1.488
Alum, (B 1)	1.488
W	1.457
Iceland spar, weakest, W	1.488
B 3	1.519
strongest W	1.657
B 3	1.665
Oil of caraway seeds, (B 1)	1.489
B 3	1.491
Tallow, cold, W	1.49
(B 1)	1.492
Oil of marjoram, (B 2)	1.490
B 3	1.491
Nut oil, (B 2)	1.491
B 3	1.507
Oil of angelica, (B 2)	1.491
B 3	1.493
Yellow wax, cold, (B 1)	1.492
W	1.542
Sulfate of iron, greatest, B 3	1.494
Balsam of sulphur, B 1	1.494
B 3	1.497
Sulfate of potass, W	1.495
B 3	1.509
Honey, (B 1)	1.495
Oil of hyssop, (B 1)	1.496
Oil of nutmeg, W	1.497
(B 1)	1.491
French plate glass, W	1.500
Treacle, (B 1)	1.500
Yolk of an egg, dry, (B 1)	1.500
Oil of rhodium, (B 1)	1.503
B 3	1.505
Oil of pimento, (B 1)	1.503
B 3	1.507
English plate glass, W	1.504
B 3	1.527
Starch, dried, (B 1)	1,504
Muriate of antimony, dry, (B 1)	1.504
exposed for two days, (B 1)	1.613
Oil of amber, W	1.505
(B 1)	1.507
Birdlime, (B 2)	1.506

Oil of fennel seeds, B 3	1.506
(B 1)	1.507
Balsam of copaiva, W	1.507
(B 1)	1.516
(B 2)	1.514
B 3	1.528
Stilbite, B 3	1.508
Oil of cummin, B 3	1.508
(B 2)	1.518
Oil of pimento, (B 2)	1.510
Scammony, (B 1)	1.510
Oil of mace, (B 1)	1.512 to 1.526
Gum arabic, W	1.514
not quite dry, (B 1)	1.513
B 3	1.512
Human cuticle, W	1.515
Dutch plate glass, W.	1.517
Myrrh, (B 1)	1.517
B 3	1.524
Tartaric acid, least, B 3	1.518
greatest, B 3	1.575
Gum dragon, B 3	1.520
W	1.66
Oil of sassafras (B 2)	1.522
B 3	1.532
W, (B 1)	1.536
Gum lac, W	1.52
B 3	1.525
(B 1)	1.528
Caoutchouc, W	1.524
B 3	1.534
(B 1)	1.557
Nitre, C	1.524
Selenite, W	1.525
greatest, B 3	1.536
Crown glass, common, W	1.525
B 3	1.534 to 1.544
Citric acid, (B 1)	1.527
Plate glass, (B 3)	1.527
Leucite, B 3	1.527
Canada Balsam, W.	1.528
(B 1)	1.532
B 3	1.549
Balsam of Gilead, (B 1)	1.529

Dry crystalline of an ox, and of a fish, W	1.530
Pitch, W	1.531
(B 1), (B 2)	1.581
	to 1.588
Sulfate of copper, least, B 3	1.531
greatest, B 3	1.552
Olibanum, (B 1)	1.532
Yellow plate glass, sp. gr. 2.52, C	1.532
Brazil pebble, sp. gr. 2.62, C	1.532
Radcliffe crown glass, W	1.536 1.533
Manna, (B 1)	1.533
burned, (B 1)	1.547
	to 1.565
Mastic, W	1.535
(B 1)	1.539
B 3	1.560
Arseniate of potass, W	1.535
Gum anime, W	1.535
(B 1)	1.546
Copal, W	1.535
B 3	1.549
(B 1)	1.553
Oil of Cloves, W	1.535
(B 1)	1.539
Spermaceti, cold, W	1.535
(B 1)	1.503
Feldspar, B 3	1.536
Oil of cashew nuts, (B 2)	1.536
Oil of anise seed, (B 1)	1.536
B 3	1.601
Gum, juniper, B 3	1.538
(B 1)	1.541
Bees' wax, W	1.542
(B 1)	1.492
Box wood, W	1.542
Benzoin, (B 1)	1.542
W	1.58
Colophony, W	1.543
Carbonate of strontia, least, B 3	1.543
greatest, B 3	1.700
Phosphoric acid, solid, B 3	1.544
Petroleum, (B 1)	1.544
Olibanum, B 3	1.544
Chio turpentine, (B 1)	1.545
B 3	1.557
Gum sagapen, (B 1)	1.545
Amber, W	1.547

White sugar, melted, (B 1)	1.545
after melting, B 3	1.555
(B 2)	1.541
W	1.535
Turpentine, (B 1)	1.545
Burgundy pitch, (B 1)	1.546
B 3	1.560
Gum Thus, (B 1)	1.546
B 3	1.554
Elemi, B 3	1.547
(B 1)	1.550
Rock crystal, W	1.547
B 3	1.562
C	1.568 to 1.575
Amber, W	1.547
Resin, (B 2)	1.548
(B 1)	1.552
B 3	1.559
Galbanum, (B 1)	1.550
Glue, nearly hard, (B 1)	1.553
Chalcedony, B 3	1.553
Rock salt, B 3	1.557
Opium, (B 1)	1.559
W	1.57
Amethyst, B 3, as crystal	1.562
Dragons' blood, (B 1)	1.565
Horn, B 3	1.565
W	1.58
Asafoetida, (B 1)	1.575
Plate glass, sp. gr. 2.76 C	1.573
Gum ammoniac, (B 1)	1.578
B 3	1.592
Flint glass, W	1.583 to 1.586
Balsam of styrax, B 3	1.584
Emerald, B 3	1.585
Oil of cinnamon, (B 2)	1.589
(B 1)	1.604 to 1.632
Tortoiseshell, (B 3)	1.591
Balsam of Peru, (B 2)	1.593
B 3	1.597
(B 1)	1.605
Guaiacum, W	1.596
(B 1)	1.600
B 3	1.619

Flint glass, B 3	1.596 to 1.616
Balsam of Tolu, W	1.60
(B 2)	1.610
(B 1)	1.627
(B 3)	1.628
Resin of jalap, (B 1)	1.608
Bluish topaz, from Cairn Gorum, B 3	1.624
from Aberdeenshire, B 3	1.636
Castor, (B 2)	1.620
B 3	1.626
Oil of cassia, (B 2)	1.624
(B 1)	1.631
B 3	1.641
Muriate of ammonia, B 3	1.625
Aloes, (B 1)	1.634
Yellow topaz, B 3	1.638
Sulphate of strontia, B 3	1.644
Red topaz, B 3	1.652
Spargel stone, B 3	1.657
Epidote, least, B 3	1.661
greatest, B 3	1.703
Sulfate of barita, W	1.646
greatest, B 3	1.664
Tourmaline, B 3	1.668
Chrysolith, least, B 3	1.668
greatest, B 3	1.685
Boracite, B 3	1.701
Axinite, B 3	1.735
Cinnamon stone, B 3	1.759
Ghrysoberyl, B 3	1.760
Spinelle ruby, B 3	1.761
W	1.812
White sapphire, W	1.768
Rubellite, B 3	1.779
Orange coloured jargon, B 3	1.782
Jargon, least, B 3	1.961
greatest, B 3	2.015
Pyrope, B 3	1.792
Blue sapphire, B 3	1.794
Arsenic, W	1.811
Carbonate of lead, least, B 3	1.813
greatest, B 3	2.084
Garnet, B 3	1.815
Sulfate of lead, B 3	1.925
Sulfur, (B 2)	2.008
W	2.04

Sulfur, native, B 3	2.115
melted, B 3	2.148
Glass of antimony, (B 3)	2.216
W	1.98
Phosphorus, (B 2)	2.125
B 3	2.224
Diamond, Newton	2.439
B 3	2.470 to 2.487
Rochon	2.755
Chromate of lead, least, B 3	2.479 to 2.503
greatest, B 3	2.926 to 2.974
Realgar B 3	2.549

V. *Note on Professor* SVANBERG'S *reduction of the Length of the* PENDULUM. *By the* TRANSLATOR *of his Paper.*

THERE appears to be a slight imperfection in the method which Professor Svanberg has employed for correcting the number of vibrations, on account of the want of adjustment of the sliding weight. He has taken the mean of two experiments, with considerable changes in the place of the weight, as attributing their difference to some accidental errors, while in fact there is no reason to suppose either of them to have been materially erroneous; and the true theoretical correction may be obtained from them, by means of an easy calculation, with less probability of inaccuracy, than by taking the mean.

Expressing the length of the equivalent simple pendulum by $l = \frac{a+pxx}{b+px} = \frac{a+pxx}{gw}$, x being the variable distance of the small weight p from the point of suspension, w the whole weight, and g the distance of the common centre of gravity from the point of suspension. Then $\frac{\delta' l}{\delta x} = \frac{2px}{b+px} - \frac{p(a+pxx)}{(b+px)^2} = \frac{2px}{gw} - \frac{pl}{gw} = \frac{p}{w} . \frac{2x-l}{g}$; which evidently vanishes when $x = \frac{1}{2} l$, the vibrations being then the most frequent. For the opposite position, in the case of perfect adjustment, we must substitute

$l - g$ for g, the other quantities remaining unaltered. It follows, therefore, that the change of the length of the equivalent simple pendulum, in consequence of a small motion of the weight, is directly as the distance of the weight from the middle of the length of the pendulum, for the same position of the pendulum; and for the opposite positions, the changes are reciprocally as the distances of the centre of gravity from the respective points of suspension.

Now it appears that in these experiments, the corresponding changes, proportional to Δ and Δ', were first 52.810 and 82.805, and secondly 71.717 and 111.284, and the respective quotients of these numbers, that is, 1.568 and 1.552, express the values of the quotient $\frac{g}{l-g} = q$; whence, since $g = lq - gq$, $g = \frac{lq}{l+q}$ $= \frac{1.568}{2.568}l$ and $\frac{1.552}{2.552}l$ respectively, or $.6106l$ and $.6080l$, the difference being $.00268l$, or about one hundredth of an inch, and the sliding weight must have been moved, from the middle point of the corresponding distances, over as many times one hundredth of an inch as its own weight p is contained in that of the whole pendulum w. It follows, also, that the distance $l-g$, or $.39l$, must have been about 15 inches. The sliding weight, in this pendulum, must have been near the great weight; and its approach to the middle must have lessened the inequality of the distances of the centre of gravity, at the same time that it accelerated the vibrations.

We may therefore conclude that, in the first series of experiments, when the difference depending on Δ was only about 1.6 vibrations, the place of the centre of gravity was such that q, instead of 1.568, must have been greater by $(1.568 - 1.552)\frac{51}{19} = .0430$, that is, about 1.611, and $\frac{1}{q-1} = 1.637$, whence the correction, instead of 1.6043, becomes 1.470 only, and $N =$ 86149.495, the difference being about 1 part in 600000: the length of the seconds pendulum is of course affected in the duplicate ratio, and must be diminished about $\frac{1}{300000}$, or one eight thousandth of an inch. If, however, the sliding weight was at the opposite end to the great weight, the ground of this computation must be abandoned, as having been derived from the errors of observation only.

The troy pound, promised by Captain Kater to the author of these experiments, has probably been received by him some time ago. T. Y.

vi. *Diagrams of the* VARIATION *and* DIP, *as observed and as computed.*

IT is not commonly known how nearly the variation of the compass and the inclination of the dipping needle may be computed for any point whatever of the earth's surface, from observations made at any one other point; it has, therefore, been thought worth while to exhibit to the eye at one view some diagrams which were drawn many years since, with a view of illustrating these phenomena; one of them has not been *published*, and the others have hitherto remained as little *known* to the public, from their being buried in an immense mass of neglected materials.

The theory, on which they are founded, requires only the supposition of the existence either of a single magnet, lying in a given direction at the centre of the earth, or of any imaginable number of equal small magnets, parallel to each other, and distributed equably over the whole surface of the earth, or in any concentric strata parallel to it. The laws of magnetic attraction and repulsion were invented by Aepinus, and the theory has been improved by Cavendish, Robison, Biot, Humboldt, and others. Captain Sabine has lately shown how well this theory, as first applied by Dr. Young to the connexion between the situation of the place, with regard to the magnetic pole, and the intensity of the force, is supported by a multitude of very accurate experiments.

Fig. 1.

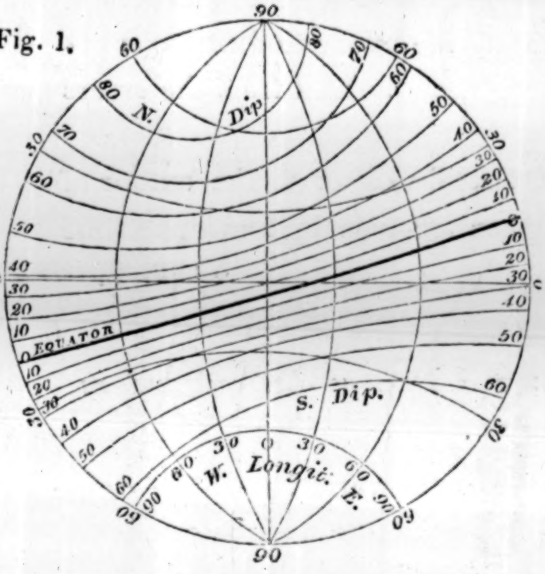


Fig. 2

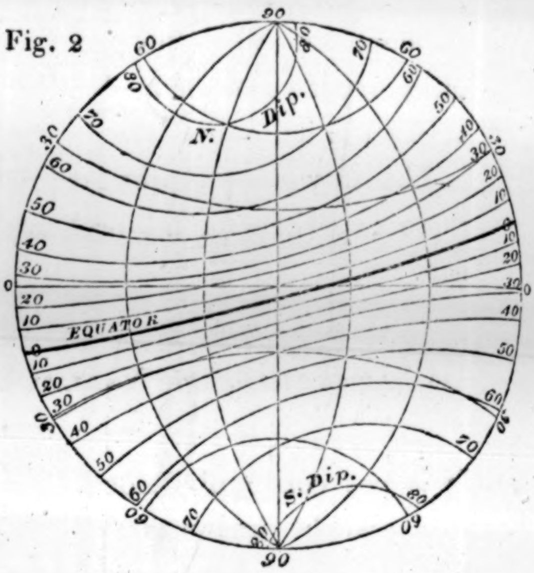


Fig. 3.

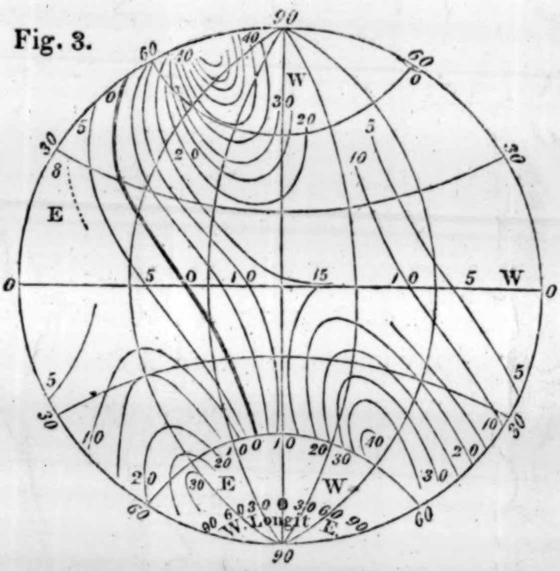
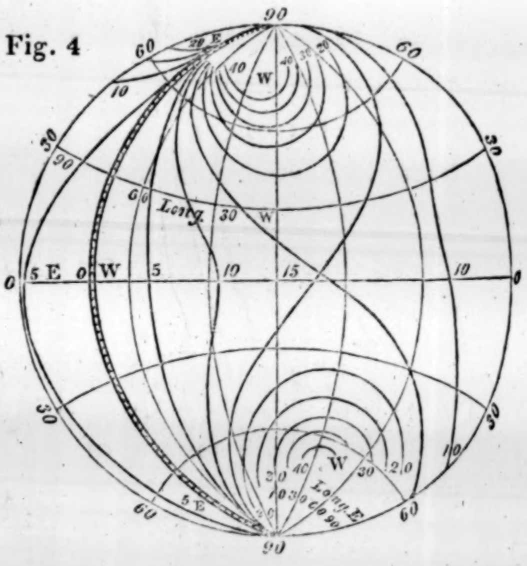


Fig. 4



The first and third figures show the dip and variation about the year 1794, as found by actual observation, for the hemisphere bisected by the meridian of London. The second and fourth exhibit the same lines, as computed for a supposed central magnet, pointing to a pole in latitude 75° N. longitude 70° W. of London. The discordance seems the greatest to the south of Cape Horn; where the direction of the general magnetism must be supposed to be materially different. Hence it has sometimes been imagined that there must be a second southern pole, having a prevailing influence in the southern hemisphere. There is, however, no reason to suppose, that any *finite* number of poles would agree very accurately with the phenomena as observed throughout the earth's surface. This question requires for its decision the computation of the direction of the general attraction for a great number of points on the earth's surface: a computation which may be performed without difficulty from the knowledge of the dip and the variation together.

The variation gives immediately the angle of the spherical triangle lying between the place of observation, the pole of the earth, and the magnetic pole; and the arc of which the cotangent is half the tangent of the dip is the measure of the distance of the latter pole from the point of observation. We have, therefore, two sides and the included angle, to find the other part of the triangle, and having the situation of the magnetic pole, the same computation inverted will give the dip and the variation for any place required.

Example of the computation.

	°	′				
L.ta. dip	82	53,7		10.90462		
				.30103		
cot.	13	59		10.60359		
Colat.	21	1	greater side			
Sum	35	0				
Diff.	7	2				
Half sum	17	30	L. cosec.	10.52186	L. sec.	10.02058
Half diff.	3	31	sin.	8.78774	cos.	9.99918
Half diff.	35	12	cot.	10.15155	cot.	10.15155
Lta. half diff.	16	8		9.46115		10.17131
half sum.	56	1				
Lesser angle	39	53	L. cosec.	10.19299		
Long. place	53	13	sin. 13° 59′	9.38317		
Long. pole	93	6	70 24	9.97408		
			cos. 69 13	9.55024	Lat. pole	

Places of the Magnetic Pole computed from various observations of Dip and Declination.

Date.	Place.	Lat.	Long.	Dip.	Decl.	Lat. Pole.	Long.
		° ′	° ′	° ′	° ′	° ′	° ′
1 1576	London.	51 31N.	0 0	71 50	11 15E.	81 37N.	47 17 E
2 1676				73 47	3 20W.	81 30	11 21 W
3 1720				75 10	11 9	76 44	23 15 W
4 1772				72 20	20 49	76 35	55 21 W
5 1820				70 6	24 12	75 13	70 26 W
6 1821	Rio Janeiro	22 54S.	43 11W.	15 26S.	3 21E.	74 38	30 34 W
7		29 50S.	36 40W.	30 52S.	1 28E.	76 45	30 31 W
8		35 7S.	0 3E.	45 14S.	20 43W.	70 25	70 24 W
9		39 2S.	6 16E.	52 44S.	23 23W.	70 21	74 7 W
10		39 36S.	15 58E.	56 0	26 56W.	68 49	73 53 W
11	St. Paul's	38 45S.	77 33E.	66 19S.	20 44W.	72 6	53 0 W
12		38 43S.	114 18E.	68 31	3 37W.	76 41	55 57 W
13	Paramatta.	33 19S.	151 1E.	62 46S.	8 44E.	77 55	60 20 W
14 1824	Whale Fish I.	68 59N.	53 13W.	82 54N.	70 24W.	69 13	93 6 W
15	Port Bowen.	73 14N.	88 55W.	87 43	123 22W.	70 22	100 20 W
					Biot	79 0	32 20 W
				1794	(Young)	75 0	70 0 W
				1825	(Sabine)	60 0	78 0 W

It appears from these computations, 1, that the place of the partial magnetic pole, as determined for any part of the world, is every where *within the polar circle*, and for London has always been so.

2. It is every where nearer to the polar circle than to the pole, its greatest latitude being under 78°, at Paramatta.

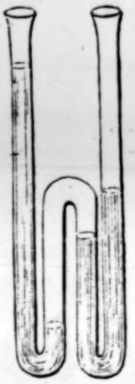
3. Its longitude appears to vary from 70° to 100°, between the stations of Port Bowen, and Rio Janeiro.

4. The variation of the place of the pole for different *times* seems to be greater than for different *places*; so that the theoretical figure, here inserted, probably gives the present declination of the whole hemisphere with less error than it does that of any one place two or three centuries ago.

Art. XIV. MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *On a Syphon Hydrometer by* Mr. Meikle.—This instrument consists of a glass tube open at both ends, and bent into a kind of double syphon, having four parallel legs; so that the open ends are pointed in the same direction or upwards, as in the annexed figure. The manner of using it is very simple. Let one of the ends be stopped with a finger or cork, and water be poured into the other. This fluid will only rise a small way into the second leg, because of the included air. Next stop the other orifice, and open the one first closed; and having poured into the latter the liquid, whose specific gravity is to be tried, open the top of the water-tube; then the instrument, being held upright, the two liquids will arrange themselves so as to press equally on the included air. This pressure will be measured by the difference in the heights of the two columns of either liquid, multiplied by its specific gravity, so that, by dividing the difference of the two columns of water by the difference of those of the other liquid, we obtain the specific gravity of the latter; that of water being unity. The difference between the columns may be measured by applying any scale of small equal parts, or the glass may be attached to a graduated plate furnished with verniers, &c. The longer the columns of liquids employed, the more accurate the process. The expansion of the glass, or its capillary action, cannot affect the result, nor is it influenced by the expansion of the scale; the only correction required will be to reduce the observations to one temperature.—*Phil. Mag.* lxviii. 166.



2. *Strength of Bone.*—Mr. Bevan finds that bone of horses, oxen, and sheep, has a cohesive strength per square inch, varying from 33,000 pounds to 42,500. One specimen of fresh mutton-bone supported a load in proportion to 40,000 lbs. per square inch for a considerable time, without any visible injury to the bone. The modulus of elasticity of beef-bone, of specific gravity 2.08, was 2,320,000 lbs.—*Phil. Mag.* lxviii. 181.

3. *New Method of lighting Theatres.*—The following plan is described as having been put into practice by M. Locatelli at the theatre La Fenice at Venice, and as having completely succeeded. By the aid of parabolic mirrors the light of many lamps is concentrated over an opening made in the middle of the ceiling of the theatre, and reflected down on a system of plano-concave lenses of a foot in diameter, which occupy the aperture, and convey into the theatre the rays of light, which arrive at them

parallel, and depart from them divergent. From the pit the lenses are alone perceived, which resemble a glowing furnace; and although the luminous focus is sufficient to light the whole of the theatre, it does not dazzle, and may be viewed without fatiguing the eyes. Besides the advantage of being more equal and mild, than that of a single luminous body, the light is more intense than that of the ancient lustre; and there is no part of the theatre in which a person cannot read with the greatest facility. The apparatus being entirely concealed, accommodates itself readily to all the changes which the representation can require.—*New Mon. Mag.* xviii. 467.

4. *Transmission of Sound.*—Lieut. Foster having occasion to send a man from the observatory at Port Bowen to the opposite shore of the harbour, a measured distance of 6696 feet, or about one mile and two-tenths, in order to fix a meridian mark, had placed a person half way between to repeat his directions; but he found on trial that this precaution was unnecessary, as he could, without difficulty, keep up a conversation with the man at the distant station. (Parry's *Voyage).—Ann. Phil. N.S.* xii. 470.

5. *New Celestial Body.*—By a letter from M. Gambard, of Marseilles, it appears, that as he was examining the heavens at the commencement of November, he discovered in the neighbourhood of *α* serpentis, a body in size and appearance very much resembling the planet Mars. As it was seen with different telescopes for four nights successively, there could be no optical illusion. From an attentive review of the heavens, taken on the nights of the 25th and 26th of November, it is concluded that this body is not visible at London. Its motion is stated to be very rapid.—*Mon. Mag. N. S.* ii. 662.

6. *Olbers on the Comet of Short Period.*—The following remarks on this comet are from a letter by Dr. Olbers to M. Schumacher, dated April 24, 1826.

"That which renders the comet observed the 27th of February, by M. Biela, worthy of all the attention of the inhabitants of the earth, is, the close approximation of its orbit to that of our globe near the ascending node of the former. I have calculated the following positions according to the ellipse of Clausen, the accuracy of which appears to be supported and confirmed by the latest observations.

"The heliocentric elongation of that point in the orbit of the comet, which most nearly approaches the terrestrial orbit, calculated from the ascending node is $+ 1° 3' 32''$; the heliocentric longitude of this point in the orbit is 7231.2; the heliocentric longitude of the point in the terrestrial orbit nearest to that of the comet is 7229.26; the distance which separates these two points is 0.0055604 of the mean radius of the terrestrial orbit, or $133\frac{1}{3}$ radii of the earth.

"This time, therefore, the comet has passed by the earth's orbit at a distance only a little more than twice the greatest distance of the earth from the moon. None of the comets which have as yet been calculated, except that of 1680, have been found so near the terrestrial orbit. The perturbations occasioned in the orbits of comets by the action of the planets, and especially by the powerful attraction of Jupiter, should make this distance vary at each revolution of the comet, and may tend as much to diminish as to increase the distance. It is therefore not impossible, but that at some time the comet shall pass at a very small distance from us, and even so near that its atmosphere may be in contact with our globe. However small the probability of such an event may be considered, becoming almost infinitely small for any particular revolution of the comet, nevertheless the mere possibility may redouble our interest in determining rigorously the actual orbit of this comet, and the perturbations to which it is liable.

"Whatever may be the extent of atmosphere to be assigned to this comet when its course shall be accurately known, nevertheless, as observed the 8th December, 1805, it appeared very considerable. On the contrary, that which resembled the nucleus, and which certainly for the greater part is not a solid body, was found of very small dimension.

"It may at least be presumed, that our approximation to it may give occasion to our observing an eclipse of the comet in the earth's shadow, and thus to decide completely the question relative to its own light. There is also reason to hope generally that this proximity will enable us to learn more accurately than has hitherto been done, the nature of these mysterious bodies."

In a later letter M. Olbers says, "It is scarcely necessary to remark, that this *possible* but scarcely probable contact between our globe and the atmosphere of the comet, need occasion no fear, if ever it take place, of any unpleasant consequence to the earth or its inhabitants. It can have only a very insignificant influence even on the climate. It is an error, in my opinion, of those philosophers who have attributed the fogs and clouds, which in the summer of 1783, covered Europe, Syria, and the north of Africa, for nearly two months, to the mixture of our atmosphere with that of a comet. Atmospheres which are so rare and transparent that the smallest stars may be seen through them, certainly cannot diminish and colour the light of the sun to the degree observed in the months of June and July, 1783."—*Bib. Univ.* xxxii. 241.

II. CHEMICAL SCIENCE.

1. *Talbot on Monochromatic Light.*—"It was discovered by Dr. Brewster that the flame of alcohol diluted with water, consists chiefly of homogeneous yellow rays. On this principle he proposed the construction of a monochromatic lamp, and pointed out its advantages for observations with the microscope. This must be considered a very valuable discovery. The light of such a lamp, however, is weak, unless the alcohol flame be very large; I have therefore made several attempts to obtain a brighter light, and I think the following is the most convenient method. A cotton wick is soaked in a solution of salt, and when dried, placed in a spirit-lamp: it gives an abundance of yellow light for a long time. A lamp, with ten of these wicks, gave a light little inferior to a wax candle; its effect upon all surrounding objects was very remarkable, especially upon such as were red, which became of different shades of brown and dull yellow. A scarlet poppy was changed to yellow, and the beautiful red flower of the Lobelia fulgens appeared entirely black. The wicks were arranged in a line, in order to unite their effect for a microscope. A common blue glass has the property of absorbing the yellow light of this lamp, however brilliant, while it transmits the feeble violet rays. If these are also stopped by a pale yellow glass, the lamp becomes absolutely invisible, though a candle is seen distinctly through the same glasses. But the most remarkable quality of this light is its homogeneity, which is perfect as far as I have been able to ascertain."

2. *On Electro-Magnetic Rotation.*—We observe, in the last Number of the *Quarterly Review*, an article on electro-magnetism, in which, at p. 248, is the following passage: "The suggestion thus thrown out by Dr. Wollaston was soon after realized by Mr. Faraday's discovery of the rotatory movements which had been predicted." This we do not understand. We had understood from Mr. Faraday's historical statement respecting electro-magnetic rotation, in this Journal, Vol. xv. p. 289, and acknowledged, we believe, to be correct, that the motion expected (not *predicted*, or published,) was that of a wire upon its own axis, and not any of those obtained by Mr. Faraday. The result anticipated by Dr. Wollaston was first obtained by M. Ampere.

3. *On the Conducting Power of Metals for Electricity. By* M. Becquerel.—Sir H. Davy is the first person who investigated the conducting power of metals for electricity. This celebrated philosopher took wires of different metals, having the same length and diameter, and sought to ascertain how much of each discharged a Wollaston's trough. He found that a silver wire, six

inches in length, and $\frac{1}{220}$ of an inch in diameter, discharged sixty-five plates; six inches of tin wire, twelve pairs of plates; six inches of platina, eleven pairs of plates, and six inches of iron, nine pairs. He discovered also that metallic wires, plunged into a fluid conductor, had a conducting power in the inverse ratio of their length, and that the effect of a wire was proportional to its mass; thus six inches of platina wire $\frac{1}{220}$ of an inch in diameter, discharged ten double plates; three inches twenty, &c.; and six wires, or one of the same length six times as heavy, produced the same effect.

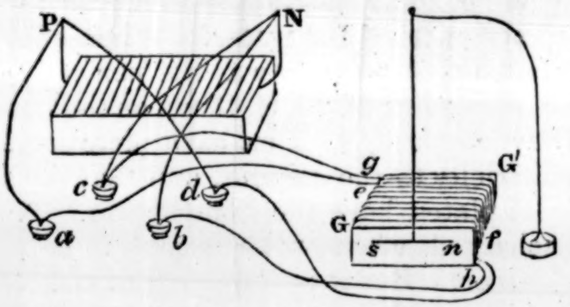
To ascertain whether a wire discharged a pile, Sir H. Davy added two other conductors to the ends of the trough, and plunged their extremities into water. When effervescence took place, he concluded that the pile was not perfectly discharged.

Such are the researches of Sir H. Davy, on the conducting power of metals. They suppose, 1. That each plate furnishes the same quantity of electricity to the current, and that piles formed of an equal number of plates have the same action. 2. That a pile is completely discharged when water ceases to be decomposed by conductors of silver. But it is known that the variations which occur every instant in the action of a pile, prevent such results from being rigorously comparable to each other, and also that the cessation of decomposition in water by conducting bodies, does not indicate the complete discharge of the pile; for all that can be concluded is, that the tension of the electricity, which does not pass through the conducting wire, is not great enough to effect chemical action. Consequently, the process described will not accurately determine the relation existing between the conducting power of different wires.

It was endeavoured to resolve the problem, by determining the conducting power of metallic bodies for electricity, independent of the continual variations in the pile. The following was the plan pursued:—Suppose that two wires of the same metal, equal in length and diameter, are attached to each of the extremities of a voltaic pile, it is evident that, if connected two and two, two currents of electricity of the same intensity will be occasioned, because every thing else is equal. Suppose also two copper wires, each twenty metres in length, and $\frac{1}{3}$ of a millimetre (0.01312 of an inch) in diameter, and covered with silk; these, being coiled round the box of a galvanometer, will leave four terminations. Let each of these ends be communicated with one of the wires, in connexion with the extremities of the pile: two currents will then pass round the galvanometer, and if the wires are so disposed as to make these move in opposite directions, the magnetic needle will remain undisturbed. Such is the first principle made use of.

The magnetic needle is, in fact, stationary whenever the contrary electric currents are perfectly equal; but this equality is not attained except the arrangement of the wires be identical with each other: this condition is obtained by making one of the four wires communicating with the ends of the pile longer than the others, and then shortening it by degrees, until the needle undergoes no deviation. To ensure the constancy of equilibrium in the needle, a line is drawn on the lower surface of the galvanometer, parallel to its direction, which parallelism is endeavoured constantly to be preserved.

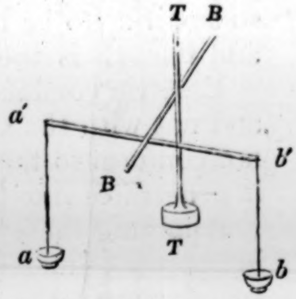
Fig. 1.



Let *P* and *N* (fig. 1.) be the two extremities of a pile; *GG'* a galvanometer; *Pa*, *Pd*, *Nc*, *Nb*, the four wires which have its extremities connected with the four capsules *a*, *b*, *c*, *d*, filled with mercury, into which are plunged also the four ends of the wire *ae*, *df*, *cg*, and *bh*, crossed so as to produce two equal currents in opposite directions. Suppose besides that the distances *ab*, *cd* are equal: if then communications are made of *a* and *b*, *c* and *d* with two wires of the same metal, equal in length and thickness, the magnetic needle will not be deviated; if at least the partial currents, which passing by *a b* and *c d* being equal in intensity, the two others which traverse the great currents are also equal. This state of equilibrium will subsist, whatever the variations in the charge of the pile; but if one of the wires *ab*, *cd*, is shorter than the other, it will allow more electricity to pass; and then the current traversing one of the great circuits *P a e h b N*, *N c g f d P*, of which two points communicate with the shorter wire, *a b*, or *c d*, will be the weaker, the magnetic needle will be deviated, and its direction will indicate which wire *a b* or *c d* allows of the passage of most electricity. This is the second principle referred to in these investigations.

It has just been said, that to ascertain the conducting power of metallic wires, it is sufficient to connect the capsules *a* and *b*, *c* and *d*, by them; but the junction requires the greatest precaution: these capsules are filled with mercury, and exactitude in the experiments requires that the wires should only touch it. For this purpose the following apparatus was used (fig. 2.): *a b* is a wire passed through the glass tube *a′ b′* fixed into a piece of cork *B B*, cut into a cylindrical form, and moving upon the glass tube *T T′*; the wire is bent towards the end. When the communication is to be made the cork is to be depressed, until the contact is established; the only precaution requisite is, to bend the wire so that the ends *a a′*, *b b′* shall reach the mercury at the same moment. Mercury is used when the experiments are made with metals not susceptible of amalgamation: in other cases the cups must be replaced by copper pincers, soldered to leaden sockets, to give them greater solidity, and made fast to the ends of the wires by sliding rings of copper.

Fig. 2.



As these experiments are comparable only whilst the temperature is the same for all the wires, temperature influencing conductibility as Sir H. Davy has shown, it is necessary to retain them constantly in a mixture of ice and water.

Suppose that *a* and *b* have been joined by a copper wire one decimetre (3.9371 inches) in length, and of any diameter: experiment proves that, to retain the needle in equilibrium, *c* and *d* must be joined by two wires of copper, of the same diameter, and double length, or with three wires of the same diameter, but triple length, &c. &c. So that to obtain the same conducting power in two wires of the same metal, their weights must be proportional to the squares of their lengths, or their lengths in the ratio of the sections of the wires. This law has been observed by Sir H. Davy.

It is thus seen that, by following a more exact method, the same results have been obtained; but it proves accurately, that the quantity of electricity which passes through two perfectly equal metallic wires is neither larger nor smaller than that which passes through one wire of the same metal and diameter, but of half the length, an advantage not supplied by Sir H. Davy's process.

It is easy to verify the preceding law in cases where the two wires of the same metal are unequal in length and diameter: let l equal the length of the first, P its weight, l' and P' the length and weight of the second; $\frac{P}{P'} = \left(\frac{l}{l'}\right)^2$, when the conductibility is the same; by experiment it appeared that a copper wire of 110 millim. (4.33 in.) weighing 0.427 grammes, and another of 34 millim. (1.338 in.) weighing 00.4 grammes, had the same conducting power; therefore $\frac{P'}{P} = \frac{0.427}{0.040} = 10.67$ and $\left(\frac{l}{l}\right)^2$

$= \left(\frac{110}{34}\right)^2 = 10.43.$ The difference between these results is so small as to be included in the inevitable errors of experiments, and the law is therefore confirmed.

Electric conducting power increases, therefore, with the mass, and not with the surface; consequently electricity in motion does not conform to the surfaces of conducting bodies as when at rest: it penetrates into their interior.

Applying these principles to the determination of the conducting power of metals, suppose a metallic wire, copper for instance, two decimetres in length, and of any diameter, conducts as much electricity as a wire of another metal of the same diameter, but one decimetre in length. It may be concluded, that two wires of the second would conduct as much electricity as one wire of the first metal, the lengths and diameters being equal. It would then be said that the copper wire had double the conducting power of the other.

When the metals may be drawn into wire, nothing is easier than to determine their conducting power by means of this process; but when, like mercury, potassium, and others, they have not this property, they must be introduced into glass tubes, perfectly uniform in their diameter, and the dimensions of the metallic cylinders thus formed, and put in communication with the wires of the circuit, accurately determined. Their conductibilities are then to be compared in the manner described.

It was found that a copper wire 61 millimetres in length, and a platina wire 10 millimetres in length, having the same diameter, had the same conducting power; a copper wire of 13 millimetres, and a silver wire of 1 centimetre, had also the same conducting power. The conclusion was, that the conductibility of copper is to that of platina as 6.1 to 1, and to that of silver as 1.3 to 1.

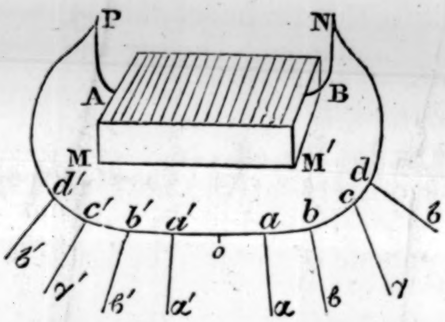
Representing the conducting power of copper by 100, the following table has been formed, representing the conducting power of other metals expressed in a function of the first:—

Copper . . .	100
Gold . . .	93.6
Silver . . .	73.6
Zinc . . .	28.5
Tin	15.5
Platina . . .	16.4
Iron	15.8
Lead . . .	8.3
Mercury . . .	3.45
Potassium . .	1.33

Annales de Chimie, xxxii. 420.

4. *On the Intensity of the Electrodynamic Force in any point of a Metallic Wire joining the extremes of a Voltaic Pile. By* M. Becquerel.—Is the intensity of this force the same in every part of the connecting wire, or does it diminish from the extremities of the pile to the middle of the wire? The experience and data acquired in the preceding investigations, will serve as guides in the examination of this important question.

Fig. 3.



Let M M′ (fig. 3.) be an ordinary voltaic pile; P A, N B, two metallic rods fixed to its extremities, and P N, the metallic wire through which the current is passing. Mark equal distances on this wire from the middle o: as oa, ab, bc, oa', $a'\,b'$, $b'\,c'$; and solder copper wires, of equal length and diameter, on to each of these points. Let a very sensible galvanometer be used, each extremity of its wire terminating in a capsule of mercury, and insert into each capsule one of the ends $a\,a$, $b\,\beta$ for example; the magnetic needle will be strongly deviated, and the deviation will be greater as the distance $a\,b$ is smaller. If the same capsules are put in communication with the ends $c'\,\gamma'$, $d\,\delta'$, so as to produce a second current in the galvanometer wire in the inverse direction of the first, the magnetic needle will not be deranged from its position of ordinary equilibrium. Hence it may be concluded, that as for two equal distances $a\,b$, $d'\,c'$, taken on a wire joining the two extremities of a pile, the two currents which traverse the wire of the galvanometer are equal, one of two things must happen according to that which we know of the division of an electric current following many conductors; either the intensity of the current is the same for every point of the wire, or it decreases in an arithmetical ratio from the extremities of the pile. This is the only means of explaining the observed results; for the difference between the intensity of the current in a and that of the current in b ought to be the same as the difference between the intensities of the currents to the points $b'\,a'$.—*Annales de Chimie*, xxxii. 428.

5. *Inflammation of Gunpowder by the Electric Discharge.*—Mr.

Howldy has been induced, by the publication of Mr. Sturgeon's letter *, to describe the method by which he has for many years been in the habit of inflaming gunpowder. A jar was placed at the prime conductor of an electrical machine; the points of the wires of the universal discharger were placed at a moderate distance from each other, with gunpowder between them, and a chain, laid upon the bare surface of the table, had one of its ends placed at the distance of four inches from the outside of the jar, and the other connected with the negative end of the universal discharger; then making the discharge through the circuit, the gunpowder was instantly inflamed. During these experiments only a small portion of the charge was transmitted, and that not as a spark. A jar containing only twenty-eight square inches of coating was sufficient for the effect.

By using a point of wood at the extremity of one of the wires of the universal discharger, and placing the extremity of the chain in *contact* with the outside of the jar, the ignition of the gunpowder was also effected; and the curious fact was observed, of the passage of the rest of the charge through the hot flame of the gunpowder as a bright spark, the heated gaseous matter assisting its transit in this form over the interval.—*Phil. Mag.* lxviii. 175.

6. *Deviation of the Magnetic Needle by the Electricity of Machines.*—Although success had attended the experiments made to magnetize steel needles and bars by ordinary electricity when in motion, yet every attempt to occasion the deviation of the magnetic needle by a current from the machine had failed, until M. Colladon instituted his experiments. Conceiving that the quantities of electricity operated with had been too small, he used a battery of thirty jars, containing 4000 square inches of surface for the previous accumulation of the electricity, and operated with a galvanometer constructed on Nobili's principle †, having 100 revolutions of wire. The wire was covered with a double thickness of silk, and the instrument placed in a room away from that containing the battery. Two copper wires covered with silk, and suspended on silk cord, connected it with the electrical apparatus. A very fine point was soldered on to each end of these wires, which were then named the *extremities of the galvanometer.*

The battery was charged: one galvanometer extremity connected with the outer coating, and the other approached towards a ball in connexion with the inner coating. When at four or five centimetres' distance (1½ or 2 in.) the needle deviated; when at less than half that distance, the deviation was 23°, and gradually diminishing continued for five seconds. The direction of the deviation accorded with the course of the electricity. Every repetition of the experiment gave the same result, and when the points brought to the battery were changed, or the charge of the battery itself was altered, the direction of the deviation corresponded with it. The quantity of deviation varied with the distance of the point; was often 40°; and the return of the magnetic needle to its original position upon the cessation of the current was in every instance ascertained.

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* See p. 20 of this volume.

† Quarterly Journal of Science, xx. p. 170.

On making experiments with a Nairn's machine, and also with a plate machine, no accumulating apparatus being used, deviations of the needle, amounting to 3° or 4°, were obtained in dry weather.

It was thus demonstrated that the electrical machine could, like the voltaic pile, produce a current competent to the deviation of the needle, and that the electricity accumulated in a given time in a battery, or even in a conductor, was a finite portion of that which circulated in the same time in a closed electromotive circuit. To establish this comparison in a more definite manner, a wire of platina was soldered to the two extremities of the galvanometer, one of the junctions heated, and the other cooled; at 125° C. (268° F.) the needle of the galvanometer deviated 45°, the same quantity which had been produced by the battery. Hence it appears that the galvanometer may, in certain cases, be a very useful indication of the quantities of ordinary electricity.

The insulation of the different turns of the galvanometer wire appearing to be an important point, M. Colladon prepared an instrument, in which the wire was covered with two folds of silk, made 500 turns round the box, and had each separated from the others by oiled silk. With this instrument the effects were nearly ten-fold what they were before, and, as in the former case, the deviation of the needle by the current from a machine was only 3° or 4°, now it amounted to much larger quantities. When the point was at

1 decimetre (3.937 in.) distance, the deviation was 18°
2 ,, ,, ,, ,, ,, 10
4 ,, ,, ,, ,, ,, 5½
8 ,, ,, ,, ,, ,, 3
1 metre (39.371 in.) ,, ,, ,, 2;

so that it was rendered sensible at the distance of a metre from the conductor.

It should be remarked that the withdrawing action of a point is sensibly proportional to its distance from the conductor. This is constantly the case with a cylinder machine, but seemed to be interfered with in a plate machine by the presence of four cushions. Experiment instituted to illustrate this point proved the correctness of this conclusion. When the motions of the cylinder machine was regular, the deviation remained constant as long as the experiment was continued.

Operating with the battery, and slowly approaching the point, a constant deviation of 30° was obtained for sixty-five seconds. A jar containing only two half-square feet of surface deviated the needle 32°.

The ratios which these experiments establish between the action of currents produced by electrical machines, and those of a pile or a thermo-electric arrangement, afford the means of appreciating the absolute velocity with which the electricity moves in a closed electromotive apparatus, when we know their electromotive force, or the tension which may be produced by the contact of two metals, and the friction of cushions. In fact, in electrical machines this velocity of circulation is determined by the motion of the glass plate by which the electricity is transported to the conductors with a known velocity. If the tension of this electricity is ten thousand times stronger than the tension of a pair of voltaic plates, having the same surface as the cushion, and nevertheless, the effects produced by the two currents on a galvanometer are the same, the velocity of circulation of the electricity from the voltaic arrangement would be evidently ten thousand times greater than that of the rubbed part of the plate; for it is an acknowledged opinion, that the deviation of the magnetic needle is proportional to the quantity of electricity which passes in the current.

Comparative experiments made with piles and thermo-electric circuits prove that the conductibility of metallic wires *is not* in the inverse ratio of their length*. When the electromotive force is small, a metallic circuit of moderate length is sufficient to stop the electric current almost entirely. The intensity of the current rapidly increases as the length of the circuit is diminished to a certain limit, dependant on the energy of the electromotive force.

M. Colladon observes also, that although bad conductors, as pure water for example, cannot be made part of a voltaic circuit without stopping the motions of the galvanometer; yet a layer of air, of more than a metre in thickness, does not always prevent this kind of action, and that the results depend upon the energy of the electromotive force; so that this becomes an important element, not to be neglected in experiments on the conductibility of bodies.

Taking advantage of some stormy weather, M. Colladon was enabled to obtain deviations of the magnetic needle by currents of atmospheric electricity, and shows that the instrument may become a very precise and useful indication of the state of this agent in the atmosphere. A metallic point being raised on the observatory of the College of France, and connected with the galvanometer, whilst the other extremity of the instrument wire was communicated with the stem of a lightning rod, deviations were obtained of 32°, 34°, and 37°. The direction of the needle indicated negative electricity, and by dismounting the apparatus, and using an electrometer, this was found to be the case. On another occasion the deviation was from 10° to 22°, and during the twenty minutes that the instrument was observed, the direction of the current changed two or three times. On another occasion the deviation amounted to 87°. These results were obtained with the first and least sensible galvanometer.

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* See p. 374, M. Becquerel on the subject.

These experiments prove that the galvanometer may be very useful in researches on atmospheric electricity. If it should be demonstrated that electricity contributes to the formation of hail, this instrument would be the only one which could indicate in a precise manner the quantities of electricity withdrawn by points more or less acute and elevated, and communicating more or less with the soil.—*Annales de Chimie*, xxxiii. 62.

7. *New Facts in Electro-Magnetism.*—M. Savary read a memoir to the Academy of Sciences at Paris, on the 31st of July, 1826, on Magnetisation by electric currents and sparks. A new fact, and results not anticipated, observed by this philosopher, seem to open a new path to the philosophers who have pursued this branch of science. One cannot see, without surprise mixed with admiration, the extent of the discoveries which have so rapidly succeeded each other since the period when Oersted first indicated the intimate union which existed between two series of phenomena, until then considered as perfectly distinct. It is a new instance of the truth of the observation, that even in those sciences with respect to which we are most inclined to think that we approach perfection, the truths unknown to us are still greater than those of which we can render a reason.

The author, commencing by a short enumeration of the discoveries made on magnetisation, states that M. Arago indicated the magnetisation produced by electrical discharges as a very simple and exact means of determining the degree of conductibility possessed by different bodies for the electricity developed by machines. He described in detail the processes devised by M. Arago, and which he has not yet published. Then arriving at the researches which are the particular object of the memoir, M. Savary gives a detail of experiments: from which it results that—

1. The *direction of magnetisation* of very small needles, submitted to the influence of an electric current passing along a straight wire, varies according to the distance of the wire. Until this time neither theory nor observation had indicated a change of this kind, except the direction of the current were also changed.

2. This action is periodical; for, when the small steel needle in the vicinity of the wire becomes magnetised in a certain direc-

tion, at a particular distance, the magnetisation diminishes as the distance of the needle is increased, until, at a certain interval, it becomes null. Continuing to remove it, the magnetisation recommences in a contrary direction, and goes on increasing with the increase of distance, until it has attained its maximum. It then diminishes as it is still further removed, at a certain distance disappears altogether, and after that resumes its powers in the first direction. This M. Savary calls a new period, and he has obtained as many as *three periods* with a single wire of copper.

3. The distances of the points at which the magnetisation is a maximum or null, varies with the length and diameter of the wire, as well as with the intensity of the discharge.

4. When a helix is used for the magnetisation, the distance at which the needle is to be placed in its interior, relative to the wire, is different, conformably to that which had been observed by M. Arago, and to that which results from the theory of M. Ampère; but then the direction and degree of magnetisation depends on the intensity of the discharge, and the relation between the length and thickness of the wire.

5. The maximum of intensity which may be produced by a given wire depends on the ratio between its thickness and length; so that the degree of magnetisation known by the name of *saturated state* is only for a certain value of this ratio: for all other ratios the maximum is less.

6. Any metal placed in the neighbourhood of the needle has a powerful influence on the direction and degree of magnetisation. M. Arago remarked, at the time of his first experiments upon magnetisation, that the interposition of glass and wood between the conducting wire and the needles did not at all change the magnetism acquired by the latter. M. Savary stated, that he learnt from M. Arago himself his intention of trying the interposition of other substances.

7. These effects vary according to the relative position of the wire, the needle, and the metal.

8. The direction of the action of the metal depends on the intensity of the discharge; so that discharges different in intensity develope in the metal a set of opposed states analogous to the polarities of contrary signs, acquired at different distances from a conducting wire, or by different intensities of electricity.—*Bib. Univ.* xxxii. 270.

8. *On a new simple Substance, Brome, by M. Balard.*–An excellent memoir, by M. Balard of Montpelier, has been read to the Academy of Sciences, and published in the *Annales de Chimie*, xxxii. p. 337. It relates to the discovery of a new elementary substance, which has received the name of Brome, and which, by its properties, appears to intervene between Chlorine and Iodine. The

memoir is necessarily of considerable length; but we have taken the liberty of condensing it very much, that we might have the opportunity of bringing it within our pages.

On adding solution of chlorine to the washings of the ashes of sea-weeds, or to the mother-liquor of salt-pans, it was observed that a yellow colour and a peculiar odour were produced: these disappeared upon exposure to air, and could not be reproduced by chlorine. They were also destroyed by alkalies and hydrogenated bodies, as ammonia, sulphuretted hydrogen, &c.; but then could be restored by chlorine. The yellow saline fluid, when distilled, gave red vapours, lost its colour and smell, and yielded a product possessing the properties, though more decidedly, of the original solution. This product evidently contained the substance which conferred the peculiar appearances noticed. To obtain it separate from the water, the red vapours were passed over chloride of calcium; they were then condensed in a small receiver, where they formed drops of a deep red colour, very volatile, filling the vessel with vapours like those of nitrous acid. Very small quantities of the peculiar substance were thus obtained, a single drop being considered as a successful result.

Every endeavour to decompose this substance, or to refer it to a known body, failed; so that, ultimately, the conclusion drawn was, that it was a simple substance, resembling, but perfectly distinct from, chlorine and iodine; and in consequence of its odour, the name of *Brome* was given to it.

Preparation.—Two processes have been resorted to for the preparation of *brome.* The first has been described. The second consisted in preparing the mother-water of salt-pans by passing chlorine through it in moderate quantities, so as not to form a chloride of brome; and to put this liquid into a flask, with a certain quantity of ether, so as to fill the vessel. Then strongly agitating the fluids for a short time, it will be found, on their spontaneous separation, that the ether has dissolved all the *brome* and acquired a hyacinth tint. Agitating the ether with caustic potash, it immediately loses its *brome*, and being again used with more of the mother-liquid, and then with the alkali, may in this way be used to separate the *brome* from a large quantity of water. The potash, gradually neutralized, became a substance soluble in water, and crystallizing in cubes. These being pulverized, and mixed with purified peroxide of manganese, and sulphuric acid diluted with half its weight of water, were distilled in a small apparatus, and gave red vapours condensing into drops of brome. These were received in a small receiver filled with cold water: the brome, in vapour, dissolved in the water; that in drops fell to the bottom and remained there, the saturated solution around it preventing its further solution. By distilling it over chloride of calcium, it could be obtained dry and perfectly pure.

Properties.—A liquid, dark red by reflected light—hyacinth red by transmitted light; odour disagreeable—intense, resembling that of euchlorine—highly sapid—acting on organic matter—corroding the skin, and colouring it yellow. It destroys animal life —a drop placed on the beak of a bird sufficed to kill it. Specific gravity 2.966—remaining fluid at 0′ Fahr.

It is very volatile—a drop placed in a vessel instantly fills it with red vapours like those of nitrous acid. It boils at 116°.6 F. —not affected by a red heat—a non-conductor of electricity, and unacted upon by that powerful decomposing agent. The vapours of brome do not sustain the combustion of a taper, but affect the tint of the flame in air. The substance dissolves in water, alcohol, and especially ether—scarcely at all in sulphuric acid, under which it may be preserved in drops. It does not redden, but rapidly destroys the colour of litmus, and also discolours the sulphate of indigo.

Hydrobromic Acid.—1. Hydrogen mixed with the vapours of *brome*, and exposed to sun-light, underwent no change; but the application of a flame, or the introduction of a hot iron rod, caused combination, and hydrobromic acid gas was produced: the combination did not take place throughout the mass, but only in contact with the hot substance, probably from want of accuracy in the proportions mixed.—Hydriodic acid, sulphuretted hydrogen, and phosphuretted hydrogen gases are decomposed by *brome*, which becomes hydrobromic acid, and evolves iodine, sulphur, or phosphorus. The action is always attended with heat. It causes no change of volume when hydriodic acid is acted upon, but produces increased volume when the other gases are used.—3. By adding sulphuric acid to the crystals formed by potash and the etherial solution before described, hydrobromic acid gas is evolved, and may be collected, but is accompanied by a small quantity of sulphurous and muriatic acids.—4. The acid was ultimately obtained pure by moistening a mixture of brome and phosphorus: it disengaged abundance of gaseous matter, which, collected over mercury, proved to be *hydrobromic acid.*

This substance is colourless—acid to the taste—producing dense white vapours in the air, having a strong and irritating odour. It is not altered, either alone or with oxygen, by a red heat: at the same time brome in vapour and water are not affected when passed through a red-hot tube. The acid is decomposed by chlorine, which produces vapours and drops of *brome*, which, being absorbed by mercury, leave muriatic acid. Pure mercury does not affect hydrobromic acid, but tin and potassium produce entire decomposition. Potassium, in the vapour of brome, diminishes it to half its bulk; becomes a white substance evolving brome by the action of chlorine; and the half-volume of gas proved, on examination, to be hydrogen. Hence this acid is analogous to muriatic and hydriodic acids in its composition, consisting of equal volumes of hydrogen and the vapour of brome uniting without condensation.

Hydrobromic acid gas is very soluble in water; heat is evolved, and the solution, when strong, evolves fumes in the air. It is colourless when pure, but dissolves *brome*, and becomes red. The application of heat drives off the free brome and much of the acid. Chlorine decomposes the solution, evolving brome—nitric acid acts upon it, and a solution resembling nitromuriatic acid is produced, capable of dissolving gold and platina. Sulphuric acid re-acts to a certain degree on the substance, and decomposes it: brome and sulphurous acid gas are produced.

Iron, zinc, and tin dissolve in the solution, evolving hydrogen. The relation of this acid to oxides is exactly the same as that of muriatic or hydriodic acid. The alkalies, earths, and deutoxides of copper and mercury form soluble compounds. The oxides of lead and silver form insoluble compounds. Oxides, which contain more oxygen than can enter into combination with them, and acids, re-act upon the substance: thus peroxide of manganese and hydrobromic acid evolve *brome*.

The affinity of *brome* for hydrogen is less than that of chlorine for the same substance, but greater than that of iodine; for hydrobromic acid is decomposed by chlorine, but *brome* decomposes hydriodic acid. For this and other reasons the acid claims a place between muriatic and hydriodic acids; it resembles the first in the difficulty of decomposing it by heat and oxygen, and the second in the action of sulphuric acid upon it, and its solvent power over *brome*.

Bromides—in every point resembling chlorides. Antimony and tin burn in contact with brome. Potassium detonates when it comes in contact with it. When thus formed by synthesis, they exactly resemble the compounds produced by acting on the hydrobromic acid by oxides. As a class they are readily distinguished, by their property of becoming yellow when treated with chloric or nitric acid, or chlorine, in consequence of the evolutions of *brome*.

Bromide of Potassium, obtained from, 1. potassium and brome, or from, 2. potassium and hydrobromic acid, or, 3. the same acid with potash, or, 4. potash with the etherial solution of brome. In the latter case small portions of the chlorides of potassium and sodium are present. However prepared, the bromide of potassium has the same properties: crystallized from its solution, it has the form of cubes, or long rectangular parallelopipeds—taste sharp—by heat it decrepitates, and then fuses without further change. Decomposed by chlorine and heat, brome is evolved, and chloride formed. Iodine has no action on it, but brome decomposes the iodide of potassium. Dry boracic acid has no action upon it at a red heat—it dissolves in water and alcohol. Sulphuric acid acts upon it, evolving brome, hydrobromic and sulphurous acid. Analysed in this way, 1.27 grains of the compound gave 0.973 of sulphate of potash: from which experiment it would result, that the bromide was composed of brome 65.56, potassium 34.44, which would give 93.26 as the proportional number of brome—oxygen being 10, or 76.14 as its number, hydrogen being 1. From hypothetical reasonings, founded on this analysis, and the known relation of volume in muriatic and hydriodic acid, it may be supposed that the specific gravity of the vapour of brome is 5.1354, and that of hydrobromic acid 2.6021.

Hydrobromate of Ammonia.—The acid and ammoniacal gases unite in equal volumes: the solutions yield a similar compound. Brome and ammonia, when acting on each other, produce the same result with the evolution of nitrogen; but no formation of a bromide of nitrogen. The salt is solid—white—crystallizing in long prisms—volatile by heat, and, when exposed to air in a moist state, becoming yellow and slightly acid.

Hydrobromate of Baryta—from baryta, with either hydrobromic acid or the etherial solution of *brome*—it fuses by heat—dissolves readily in water and alcohol. Its crystals occur in opaque mammellated masses, not resembling the muriate of baryta.

Hydrobromate of Magnesia—uncrystallizable—deliquescent—decomposed by heat.

Bromide of Lead—precipitated from a solution of lead by hydrobromic acid—fusing by heat—concreting, when cooled, into a yellow mass. When first precipitated, decomposable by nitric and sulphuric acid: when fused, requiring boiling sulphuric acid to produce this effect.

Deutobromide of Tin.—The solution of tin in hydrobromic acid, evaporated to dryness, yields a protobromide of tin. Tin, in contact with brome, burns, and becomes a solid, white, crystalline compound, readily fusible and volatile, yielding slight vapours in moist air, and dissolving in water to form an acid *deutobromate.* Heated under sulphuric acid, it fuses into drops, but suffers no further change; but nitric acid evolves *brome.*

Bromides of Mercury.—An alkaline hydrobromate, added to protonitrate of mercury, produced a white precipitate resembling calomel. When mercury was acted upon directly by *brome*, a white substance resulted—volatile by heat—soluble in water and alcohol, and producing a red or yellow precipitate with acids.

Bromide of Silver—produced by precipitation from the nitrate—yellow when dried in the shade—blackening in sun-light—insoluble in water or nitric acid, and soluble in ammonia—fusing by heat, and, upon cooling, producing a reddish-yellow hornlike substance—decomposed by nascent hydrogen. A weighed portion of the *bromide* being mixed with zinc and dilute sulphuric acid, was thus decomposed: the resulting proportions were as follows:—silver .589, brome .411.

Bromide of Gold.—Brome and its aqueous solution dissolve gold. A yellow bromide is obtained, staining animal substances of a violet colour, and being decomposed into brome and gold by heat.

Bromide of Platina.—This metal dissolves in bromo-nitric acid, and forms a yellow compound, decomposed by heat, and giving yellow precipitates, with salts of potash and ammonia.

Action of Brome on Oxides.—Passed over potash, soda, baryta, and lime, at a red heat, it combines with their metallic bases, and evolves oxygen. It did not act in the same manner with oxide of zinc. The carbonates of the above substances were affected in a similar manner. When the *brome* was made to act on the same bases in a moistened state, no oxygen was evolved; but the odour and colour of the *brome* disappeared. The compounds destroyed the colour of litmus, had the *brome* evolved by nitric acid, and were, in fact, analogous to the chlorides of lime, soda, &c. Concentrated solution of potash with brome being agitated with ether, and the latter fluid evaporated, acicular crystals were obtained, not of a hydrobromate of potassa, but of a true *bromate.* The same effects were produced with baryta and lime.

Bromic Acid and Bromates.—When the quantities in which the solution of potassa and the brome act are sufficient, the hydrobromate formed remains in solution, whilst a crystalline powder falls. This powder scintillates on a hot coal like nitre—by heat it evolves oxygen, and becomes a bromate of potassium. It is, therefore, a bromate of potassa. It is but slightly soluble in alcohol, readily soluble in water, crystallizing in needles. When mixed with sulphur, it detonates by a blow. The solution of the bromate precipitates nitrate of silver white. It does not precipitate salts of lead. It precipitates proto-nitrate of mercury yellow-white.

Sulphurous acid, sulphuretted hydrogen, and hydrobromic acid, act on the *bromate* of potash, evolving *brome*. Muriatic acid disengages a compound of chlorine and *brome;* but no compound analogous to euchlorine has as yet been obtained. Dilute sulphuric acid, acting on the salt at 212° Fahr., disengaged a mixture of brome and oxygen.

By combining brome with chlorine, and putting the aqueous solution to potash, action instantly takes place, which terminates in the production of a muriate and a bromate of potassa; these salts being unequally soluble are easily separated. A *bromate of baryta* thus obtained formed acicular crystals, soluble in boiling-water; slightly soluble in cold-water, and producing vivid combustion on hot charcoal. By adding sulphuric acid to a solution of the salt, a weak solution of bromic acid was obtained. When this was carefully evaporated, a syrupy substance was left. When heat was applied to dissipate the rest of the water, part of the acid sublimed, and the rest separated into oxygen and *brome*.

Bromic acid in solution has scarcely any odour, has a very acid but not caustic taste, reddens, and gradually destroys, the colour of litmus. Nitric and sulphuric acid exert no action upon it; but sulphurous acid, sulphuretted hydrogen, hydrobromic, muriatic, and hydriodic acids decompose it. Bromic acid precipitates salts of silver and lead white; the precipitates are bromates, that of lead is soluble in water. By decomposing a given weight of bromate of potash by heat, the loss furnished data, from which the composition of bromic acid was determined to be: *brome*, 64.69; oxygen, 35.31; which approaches to the composition that might be deduced from the former experiments.

Chloride and Iodide of Brome.—By passing chlorine through *brome*, and condensing the vapours at a low temperature, a reddish-yellow fluid chloride of brome was obtained, having a penetrating odour and disagreeable taste. It was very fluid and volatile, its vapours being yellow. It occasioned the combustion of metals; was soluble in water, and gave bleaching fluid. With alkaline solutions it gave chlorides and bromates.

Iodine appears to form two compounds with *brome*. In certain proportions a solid is obtained, which, heated, yields reddish-brown vapours, condensing into dendritic crystals. A further addition of brome converts these crystals into a dark-coloured liquid body. The latter substance is soluble in water, yielding a bleaching fluid, and with alkalies yields *bromides* and iodates.

Bromides of Phosphorus.—Phosphorus and brome made to act on each other in an atmosphere of carbonic acid gas formed two compounds—one, which was solid, sublimed and crystallized on the upper part of the vessel, a fluid substance remained beneath, and appeared to contain less brome than the former. It remained fluid at 10° Fahr., did not redden litmus, or scarcely; was readily volatile, forming pungent vapours. It dissolved excess of phosphorus, acted powerfully on water, producing much gaseous hydro-bromic acid; the fluid evaporated burnt into phosphoric acid. The deuto-bromide of phosphorus was a yellow solid, readily made by adding *brome* to the previous compound; easily fusible and vaporizable; by refrigeration both the vapours and fluid crystallized. By contact with the air it fumed, decomposing the water in it, and producing *hydro-bromic* and phosphoric acids. Chlorine decomposed these compounds, producing *brome* and chloride of phosphorus; but iodine had no action of this kind.

Bromide of Sulphur—made by adding sublimed sulphur to brome; a reddish liquid, producing fetid white vapour in the air; producing strong acids with water; decomposable by chlorine.

Hydro-carburet of Brome.—Brome introduced into olefiant gas instantly became a colourless, heavy liquid, having no odour of brome, but that of the hydro-carburet of chlorine. It was readily volatile; at a red heat it gave hydro-bromic acid and carbon. A bromide of carbon has not been obtained from it. It often happens that when distilling the mother-liquors, as before described, the brome is in part or entirely converted into this compound.

The action of brome on organic substances has been already referred to, and is perfectly analogous to that of chlorine and iodine.

As to the sources or natural history of brome:—it occurs in minute quantities in sea-water, where it appears to be combined as hydro-bromic acid with magnesia. The minerals and vegetables of the sea also contain brome. The ashes of the plants which grow in the Mediterranean, all furnish a yellow tint when their lixivia is treated with chlorine; and the ashes of the *Ianthina violacea* produced the same effect. Notable quantities of brome have been obtained from the mother-liquor of the *Soude varech*, which is used for the extraction of iodine. Finally, M. Balard has been induced to suspect its presence in a mineral-water from the eastern Pyrenees.

A report has been made on this memoir by MM. Vauquelin, Thenard, and Gay-Lussac, in which they speak very highly of the paper, and the experiments which it details; state that they have had the opportunity of examining a minute quantity of *brome*, and also of obtaining it from the mother-water of the salt-marshes of Aren, by M. Balard's process. And though they refrain from stating their conviction that brome is a new simple substance, consider it as highly probable, and a point that can only be determined by continued researches, and on larger quantities.

9. *Spontaneous Combustion of Lamp-Black.*—Extract from the ship *Catherine's* log, 3rd Feb. 1826. Lat. 1° 37′ N., long. 86° 55′ E. At 1 P.M. a strong smell of burning, and an appearance of smoke, as if rising from the fore-hold, was observed by some of the people between decks; this was immediately reported to the officer on the quarter-deck, in consequence of which the fore-hatches and fore-scuttle were taken off, when a suffocating smell of fire and clouds of smoke began to issue from both places. On going into the fore-hold, and clearing away the goods near the hatchway, found that a large cask of lamp-black in the starboard wing had taken fire, and was giving out dense columns of smoke; the cask, although not in a blaze, was too hot to be handled. All the ship's company and passengers were instantly employed in handing down water and wet blankets, the latter being found of the greatest use in stifling the smoke: these enabled the officers and people in the hold, who were in defatigable intheir exertions,

to remove the surrounding articles, chiefly large jars of linseed and neat's-foot oil, which were immediately hoisted on deck by the prompt assistance of the passengers, and at the same time a constant supply of water passed down the hatchway; and although the people in the hold were frequently driven back by the strong suffocating smell, they at last succeeded in getting the cask which was on fire, and muffled by wet blankets, brought to the hatchway: this was instantly hoisted on deck and thrown overboard before it had completely ignited, or burst into a flame; had it done so in the hold, instant destruction must have inevitably followed, it being surrounded by 200 barrels of tar, and upwards of 80 large jars of oil. As no apparent cause could be assigned for this catastrophe, as no leak, either from the deck, or from any of the jars, could be perceived, and as no light had ever been suffered in the hold since leaving England,—it was reasonable to conclude that spontaneous combustion must have taken place in the cask. And as there were many more casks of the same material on board, it was considered absolutely necessary, for the safety of the ship and cargo, as well as the lives of the crew and passengers, to throw the whole overboard.

Employed during the rest of the day in hoisting up and throwing overboard the remaining casks of lamp-black, sixty-one in number.

N.B. Two other casks of lamp-black were observed to smoke while floating past the ship.—Ind. Gaz. Mar. 20.—*Phil. Mag.* lxviii. 309.

10. *Preparation of Pure Carbonic Oxide.*—The decomposition of oxalic acid by oil of vitriol into a mixture of equal volumes of carbonic oxide and carbonic acid is well known. M. Dumas proposes to resort to this process for the preparation of the former gas. Salt of sorrel is to be mixed with five or six times its weight of concentrated oil of vitriol in a flask, and raised to ebullition; a considerable quantity of gas will come over, containing equal volumes of the two above-named. After absorbing the carbonic acid by potash, the carbonic oxide is obtained in a pure state. This process may be successfully applied in the examination of the salt of sorrel of commerce. In fact, the bi-tartrate of potassa, acted upon in the same way, yields oxides of carbon, sulphurous acid, and carbonic acid; and the fluid becomes black in consequence of the deposition of carbon. Pure salt of sorrel, on the contrary, never affords sulphurous acid, and the sulphuric acid employed remains perfectly limpid and colourless.—*Ann. de Chimie*, xxxiii. 110.

11. *New Compound of Hypo-nitrous and Sulphuric Acids.*—The description of this compound, as given by Dr. Henry, was inserted

in the last volume of this Journal, p. 391. Mr. Scanlan has observed the incidental formation of the same substance during the distillation of nitric acid. He thus describes its production and appearance:—" A few days since, while preparing nitric acid from one atom nitre, and two atoms oil of vitriol, sp. gr. 1.812, a compound was formed, which I believe to be the same as that found in the Manchester vitriol-chamber, and examined by Dr. Henry. The distillation was performed in a cast-iron pot, with a stone-ware head and connecting-pipe, to which was adapted a glass-receiver. When about nine-tenths of the acid had distilled over in a continuous stream*, the receiver was changed; it now began to drop very slowly, and was quite green†; the fire was then urged, and suddenly the receiver became lined with a white substance, which at first I mistook for the boiling over of the fused bi-sulphate of potash—an accident which has more than once occurred with me; but on examining more closely, I found the substance to be translucent and crystalline, resembling ice as it forms on the pane of a window; and I observed, when it came in contact with the liquid acid, it effervesced violently, and the acid did not become muddy.

" Although I could not collect enough of the substance to prove its identity with that described by Dr. Henry, yet I think there is little doubt that they are the same. A small portion crystallized in the bent tube connecting the receiver with a Woulfe's bottle; when distilled water was introduced into this tube, nitrous gas was evolved the instant the water came in contact with the substance, with brisk effervescence, first becoming blue-green at the point of contact; and the resulting solution, which was transparent and colourless, abundantly precipitated solution of nitrate of baryta.

" I conceive that the production of this compound, in the present instance, can only be accounted for by supposing that, when the nitric acid had distilled over, the atom of sulphuric acid, constituting a bi-sulphate, began to act upon the iron, producing sulphurous acid gas, which, coming in contact with nitrous acid in the atmosphere of the apparatus, gave rise to this substance. The production of sulphurous acid gas would also account for the boiling over before-mentioned; for I find bi-sulphate of potash to fuse quietly in a glass retort, and to bear a much higher heat than I have ever applied in making nitric acid without parting with any of its acid."—*Ann. Phil.* N. S. xii. 339.

12. *On the Decomposition of Fulminate of Silver by Sulphuretted Hydrogen, by* M. Lieberg‡.—If sulphuretted hydrogen be passed

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* Specific gravity 1.455 did not disturb solution of nitrate of baryta.

† Specific gravity 1.237 contained a good deal of sulphuric acid.

‡ See vol. xvii. of this Journal, p. 153, 386.

through water holding fulminate of silver in suspension, and the liquid be strongly agitated before the fulminate be entirely decomposed, an extremely penetrating odour will be remarked; and ammonia, if brought into the neighbourhood, will cause white fumes. As soon as the salt is entirely decomposed, which is indicated by the limpidity of the liquid, the odour is no longer observed.

The liquid, when filtered from the sulphuret of silver, has an austere taste, and reddens litmus; when mixed with lime, ammonia is disengaged; being heated with nitric acid, it will precipitate the muriate of baryta, produces an abundant yellow precipitate with nitrate of silver, and changes the colour of salts containing the peroxide of iron to a deep red. Hence it appears that the cyanic acid in the fulminate decomposed by sulphuretted hydrogen is changed into cyanite (?) of ammonia, and a particular acid containing sulphur, but different from the sulpho-cyanic acid. The liquid exposed to air for some time deposits a yellow powder, and the odour of hydrocyanic acid then becomes sensible. When concentrated by evaporation, it yields a deliquescent ammoniacal salt, which, upon the addition of acids, evolves the penetrating odour of sulpho-cyanic acid.

As it appeared probable that the formation of the ammonia had been determined by the affinity of the acid, sulphuret of barium, obtained by heating sulphate of baryta and lamp-black together, was used to decompose the fulminate in another experiment. The sulphuret of barium was added, little by little, to the fulminate, suspended in boiling water as long as any sulphuret of silver was formed. The filtered liquid was very alkaline, but gave no hydrocyanic acid upon the addition of other acids; the nitrate of silver produced a yellow precipitate, becoming black when dry. Carbonic acid gas passed through the liquid produced but a very small quantity of carbonate; and by evaporation a yellow salt was obtained, which, heated to 212° Fahr., burnt without light at the moment when it lost the last portions of water, and became grey. Then acted upon by water, that fluid dissolved sulpho-cyanuret of barium, and carbonate of barytes remained. Acids disengaged sulpho-cyanic and carbonic acids, and lime evolved ammonia. Heated, when dry, in a glass tube, it fused, gave carbonate of ammonia and cyanogen, and sulphuret of baryta remained.

When nitrate of silver is precipitated by the salt of baryta recently prepared, a voluminous yellow precipitate is obtained, which, when well washed and heated with water to 212° Fahr. becomes changed into sulphuret of silver and carbonate of ammonia. From this it appears that the acid which is united to these oxides ought to contain oxygen, as well as carbon, hydrogen, and nitrogen.

On decomposing the salt of baryta by sulphuric acid, an acid liquid is obtained, decomposing with great facility. If the salt be pure no particular product is observed, but if it contains even the slightest trace of silver, hydrocyanic acid is disengaged.

If the liquid produced by the decomposition of fulminate of silver by sulphuret of baryta be filtered before all the fulminate be decomposed, fulminate of silver and crystallized baryta is obtained upon cooling, which proves that the fulminate loses half its oxide before the fulminic acid itself suffers any change.

If the fulminic acid gives half its oxygen to the sulphuret of barium, and in exchange takes a corresponding quantity of sulphur, the new acid should be formed of two atoms of cyanogen, one of sulphur, and one of oxygen; and the salt of silver, when decomposed by heat with six atoms of water, would produce one atom of sulphuret of silver, four of carbonic acid, and two of ammonia.

Though these results are not so accurate as might be desirable, they nevertheless prove that, in the decomposition of fulminate of silver by sulphuretted hydrogen, or sulphuret of barium, other products are formed than those which were supposed; and that the red colour produced by the liquid when mixed with a salt of peroxide of iron, is not a sufficient proof of the existence of sulphocyanic acid, since there are several other bodies entirely different from this acid which possess the same property—*Annales de Chimie*, xxxii. 316.

13. *On the Preparation of Pure Nickel. By* M. Berthier.—The following considerably-abridged instructions from a paper by M. Berthier, relative to the preparation of pure nickel, will have an interest, from the attempts that are making to introduce a white alloy of nickel with copper and other metals, intended in many of its applications to replace silver.

If speiss or impure nickel be heated with four or five times its weight of lead on a cupel, it fuses, floats on the lead, without entering into combination, is gradually roasted, and the oxides formed, dissolved by the litharge, and carried over the edges of the cupel. It gradually loses fusibility, and becomes solid at a high temperature; being allowed to cool, and plunged in water as soon as the remaining lead is solid, it is easy to detach a plate of speiss, and to separate the scoria from it, especially by acting ultimately upon it with a little common muriatic acid. The new speiss is an arseniuret, free from sulphur, and contains no cobalt.

When large quantities are operated with, the roasting is long, and may be replaced by the action of litharge. The speiss (A) in powder is to be mixed with twice its weight of litharge, and heated rapidly in a crucible. The mixture fuses, becomes very fluid, and ultimately there is obtained a button of lead, a new speiss (B), and scoria of a compact appearance, greyish-black colour, and shining fracture. Generally the speiss contains a small quantity of cobalt, which may be removed by refusion with one or two parts of litharge, both being in powder, and heated as before. The speiss (C) is then an arseniuret of nickel: the loss in the fusion being from 40 to 50 per cent. The scoria should not be rejected, but have the nickel and cobalt they contain separated in the humid way.

Nitre produces nearly the same effect on the speiss as litharge: 100 speiss and 40 nitre being mixed, and thrown into a hot crucible, became incandescent, swelled, and ultimately by raising the heat fused perfectly; a speiss (F), weighing 84 grains, was obtained; 50 parts of this with 20 of nitre, heated, gave a result containing the metal in grains, which, when washed out, equalled 35 parts (G); and 30 parts of this speiss with 12 of nitre, acted upon in a similar way, gave 18 parts of a pure speiss. The total loss in these operations was 68 parts, but was reduced in other operations to 50 parts, by employing less nitre in the third operation.

The speiss is thus brought into the state of a pure arseniuret of nickel, and is then to be treated in the following manner: pulverise, roast until arsenical vapours cease to ascend; add one-tenth of metallic iron; digest in common nitric acid, to which a little muriatic acid is now and then added; evaporate slowly to dryness, and re-dissolve in water. The greatest part of the arseniate of iron will remain undissolved; the minute quantity held in solution may be precipitated by carbonate of ammonia, added drop by drop, and not in excess: then pass sulphuretted hydrogen through the liquid, or add successively a little nitric acid and hydrosulphuret of ammonia, to precipitate the copper and lead which may be present: evaporate to dryness, and calcine the residue at a white heat; a perfectly pure protoxide of nickel will be the result. No fixed matters being used in this process, washings are dispensed with, and large quantities may be operated upon.

The roasting is the troublesome and disagreeable part of the process just described, but may be avoided, by using litharge or nitre, and proceeding as above. To oxidize all the arseniuret, eight or ten times its weight of litharge must be employed; but if nitrate of lead be used, a much smaller quantity will suffice; the solution of the scoria will be more rapid, and require less acid. The speiss of commerce requires two parts of nitre, but the arseniuret of nickel only one and a half parts: as the action is rapid, it must be tempered by adding two parts of carbonate of potash or soda. This addition assists in removing the arsenic from the oxide of nickel. After the matter has been washed, the iron is to be added, the solution made in nitric acid, and the oxide of nickel finally precipitated by carbonate of soda, and washed. If the intention is to prepare metallic nickel, the washing is unnecessary. After separating all the arseniate of iron and the copper, it is to be evaporated to dryness, raised to a white heat, the oxide coarsely washed, and reduced in a crucible lined with charcoal, with the addition of some glass.

When the pure oxide of nickel has been obtained, it is reduced by heating it very highly in a crucible lined with charcoal, mixed with a small quantity of glass. If large buttons are required, nine or ten per cent. of pulverized charcoal must be added to the oxide, but the metal then contains a small quantity of carbon. It can only be obtained perfectly pure by reducing the oxide by hydrogen gas. If a quantity of the oxide be strongly pressed into a crucible lined with charcoal, and heated to whiteness, the metal is reduced by way of cementation. The nickel is then porous and grey, but becomes highly brilliant by burnishing. It flattens under the hammer, and may be welded and drawn into bars at a high temperature. When the metal is to be prepared in large masses, the oxide is to be intimately mixed with .08 or 0.1 of charcoal in powder, strongly compressed in successive layers, into a naked crucible heated to whiteness, for a sufficient time, and then being sprinkled over with borax, forged at that high temperature.—*Annales de Chimie*, xxxiii. 49.

14. *Mosaic Gold.*—An alloy which has received this name, in consequence of the resemblance which it has in colour to gold, has been made the subject of a patent, and is directed to be thus prepared. Take equal quantities of zinc and copper, and melt them at the lowest temperature at which copper will fuze. Having mixed them perfectly by stirring, add zinc in small portions till the alloy in the crucible assumes a yellow colour like brass: then continue adding the zinc till the colour changes to a purple or violet, and becomes perfectly white, which is the colour necessary for its perfection. It may then be cast into ingots, or into any required form, and when cold it will have the appearance of an alloy of pure gold and copper. The great art is to work at the lowest possible temperature, and it is difficult to re-melt the alloy without altering its character by dissipating part of the zinc. It consists of 52 parts of copper, and from 48 to 50 of zinc.—*Edin. Journal.*

15. *On fusible Metal and a refrigerating Metallic Combination.*—M. Dobereiner says, "the alloy composed of lead 340 parts, tin 194 parts, and bismuth 466 parts, is fusible at 210° F. It may be considered as composed of one atom of an alloy of bismuth and lead, fusible at 323°.6, or 336°.2 F. united to one atom of an alloy of bismuth and tin, which fuses at 267°.8 or 278°6 F. When

the combination of these alloys takes place, it produces cold. The following is a more striking example. Having mixed 207 parts of lead, 118 of tin, 284 of bismuth, and 1617 of mercury at the temperature of 63°.5 F., the thermometer instantly descended to 14° F.—*Annales de Chimie*, xxxvi. p. 334.

16. *Sulphuret of Cerium.*—Sulphuret of Cerium may be prepared, according to Dr. Mosander, in the following manner. Carburet of sulphur is to be passed in vapour over the carbonate of cerium heated to redness; the sulphuret of cerium produced is red, resembling minium in appearance—porous—light—not alterable either in air or water. When oxide of cerium is fused with sulphuret of potass in great excess, at a white heat, a sulphuret is also formed, which may be obtained separate by washing. It appears as small brilliant scales resembling mosaic gold in powder, but, under the microscope, appearing to be transparent and of a yellow colour. These apparently different varieties of the substance dissolve readily in acids, evolving sulphuretted hydrogen gas, and yielding no residue of sulphur. The compound consists of 74 parts cerium, 26 parts of sulphur.—*Annales de Chimie*, xxxiii. 109.

17. *On the formation of Pyrites in a Mineral Water.*—M. Barbier, the mayor of Chandesaigues, brought to Paris a deposite formed by the hot mineral waters of that town, and placed some of it in the hands of M. Longchamp, who has published a brief note upon the subject. The deposite is found in a channel about three metres in length, connecting the principal source of water called *Du Parc* with a small reservoir. This channel is about eight inches square, and would become choked in three or four years, were not the precaution taken of cleansing it. It is only in this distance that the deposite is formed, that which is produced farther off having other properties. The substance is not formed in layers, but is mammellated and divided, probably by the course of the water through its mass. Its surface is reddish-brown, and appears to be only oxide of iron; but, when broken, the substance presents gray and brilliant plates, which could not be cut by the point of a knife, and which were recognised by their appearance for sulphuret of iron. When fragments were placed upon ignited coals, the blue flame of sulphur appeared, and the odour of sulphurous acid was developed.

Chandesaigues is placed in the large circus bounded on the west and north by the volcanic group of Cantal, and to the south by the mountains of Aubrac, also volcanic. The primitive formation which constitutes the plains of Auvergne, is here and there exposed, to a considerable extent, and particularly near Chandesaigues: it is there composed of yellow felspar, gneiss, and gray argillaceous mica-slates, which sometimes contain pyrites, and are often covered with efflorescent sulphurets. It is from the middle of these rocks that the sources of water spring out, and, according to M. Barbier, they pass through the cavities in quartz veins, the cavities being lined with sulphuret of iron. The temperature of the water is 88° centigrade, or 190° 4 F.

No conclusions of any force are drawn relative to the mode in which the pyrites is formed: it is considered as coming from the interior, and, indeed, as being formed from substances held in solution by the water; but the analyses hitherto made of the water do not indicate the presence of substances competent to furnish this compound. The oxide which covers its surface is considered as a result of the decomposition of pyrites previously deposited, and not as a deposite from the water, formed at the same time with it.—*Annales de Chimie*, xxxii. 294.

18. *Anhydrous Sulphate of Soda.*—Dr. Thomson has examined certain crystals formed in saturated leys containing mixed sulphate and carbonate of soda, &c., and has found them to be a true anhydrous sulphate of soda. They were octohedrons, with a rhombic base, of a very large size, many of them measuring 1.8 inches in length and 0.8 in breadth. They were translucent, but not quite transparent: the faces were rough. They were firm and solid, had a glassy appearance, and suffered no change by an incipient red heat, except the loss of a minute quantity of water. They absorbed moisture slowly, and effloresced in a damp air. They fused at a high red-heat. 100 parts of water, at 57° F., dissolved 10.58 parts, and, upon cooling, gave an abundant crop of common crystals. The specific gravity of the anhydrous crystals was 2.64. When analysed, the results indicated the composition of the salt to be the same as that of Glauber's salt, with the exception of the water.—*Ann. Phil.* N.S. xx. 401.

19. *Purification of Crystals; by* M. Robinet.—Every practical chemist knows how difficult it often is, particularly in the analysis of organic substances, to clear away from crystalline products the mother-water and other heterogeneous matters which collect in their interstices. When the crystals are very fine, and still more, when they are soluble in the ordinary menstrua, it is sometimes impossible to clear them, although perfectly pure, by any other method than repeated crystallization, and digestion with animal charcoal; both of which processes are troublesome, and occasion considerable loss. M. Robinet has proposed a new and very simple method, which was suggested to him in consequence of observing, that when a parcel of crystals came into contact with the mouth of the *pipette*, during the act of suction, they were instantly and perfectly cleaned. The process depends on the

transmission of a current of air through the crystals. He has suggested various forms of apparatus for the purpose: the simplest consists of a double-mouthed bottle, with a funnel in one mouth and a bent tube in the other, the lower opening of the funnel being obstructed by a ball of cotton-wool, and the crystals placed above the cotton. On sucking the air through the crystals by a bent tube, they are cleaned in a few seconds; and, if necessary, the operation may be repeated, previously introducing a little water into the funnel. A convenient way of constructing the apparatus, so as to work of itself, is to make the bent tube reach the bottom of the bottle with one limb, and with the other a vessel of water situated on a lower level. The whole bottle and tube being filled with water, the funnel is to be introduced, and the water then allowed to run off by the syphon. On the large scale, a more suitable apparatus will be a tube from a steam-boiler, by which the bottle may be filled with steam from time to time. The steam communication being shut off, and the steam in the bottle condensed, the stream of air will immediately carry through with it the whole of the mother-water from the most silky crystals.—*Ann. Phil.* N.S. xii. 460.

20. *Note by M. Vicat relative to his last Memoir on Mortars.**—It is not true that clays slightly calcined with contact of air decompose more limewater than when in a natural state; but the contrary. But it is certain that their energy is sensibly proportional to the quantity of limewater which they decompose.

Good puzzolana clays, reduced to powder, and calcined on an incandescent plate, decompose 260 parts of limewater saturated at common temperatures, for every 100 parts of the clay, in less than one hour

The same clays, when in powder, calcined for two hours in close vessels, only decomposed 100 parts of limewater for 100 of the calcined substance.

Those clays which furnished only very moderate puzzolanas, decomposed, in the same circumstances, from 60 to 80 of limewater, and the worst varieties only from 25 to 38 parts of limewater. The Italian puzzolanas decomposed 147 parts of limewater per cent. of powder used.

Those clays which, when, in a natural state, will not decompose from 400 to 500 parts of limewater per cent. of powder used, cannot be employed as puzzolanas. The clays extracted by washing from *arenes* of the best quality, decompose as much as 1100 parts of limewater per cent.—*Annales de Chimie*, xxxii. 443.

21. *Re-action of Sulphate of Magnesia and Bi-carbonate of Soda.*—M. Planches has observed, that when bi-carbonate of soda and sul-

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* See the last volume of this Journal, p. 214.

phate of magnesia are mixed together in the state of powder, and kept for a long period, mutual action takes place, and the mixture is no longer entirely soluble in water, as happens with the recent preparation. Hence the impropriety of reserving these substances in a state of mixture for more than a certain period, which appears to be about five or six months.—*Journal de Pharmacie.*

22. *Sympathetic Ink.*—The following application of a modern chemical discovery has never before been communicated to the public, and affords a sympathetic ink very far superior to any as yet in use. Dissolve a small quantity of starch in a saucer with soft water, and use the liquid like common ink: when dry, no trace of the writing will appear on the paper, and the letters can be developed only by a weak solution of iodine in alcohol, when they will appear of a deep purple colour, which will not be effaced until after long exposure to the atmosphere. So permanent are the traces left by the starch, that they cannot, when dry, be effaced by Indian rubber; and in another case, a letter, which had been carried in the pocket for a fortnight, had the secret characters displayed at once by being very slightly moistened with the above-mentioned preparation.—*New Monthly Mag.* xviii. 420.

23. *Adulterated Sulphate of Quinia.*—The salt, which had been offered for sale at a low price, was less white than the genuine sulphate; its crystals shorter and less acute; its odour resembling that of rancid fatty matter; its taste less bitter, but more acrid, and producing an irritation that remained for some time in the throat. Upon placing 14 or 15 grains of the preparation in a silver spoon, and slowly heating it, it divided into two portions, one fusible at a low temperature, the other infusible. The fusible portion gave the same appearance to paper that fatty matter does, and was stearine; the infusible was pure sulphate of quinia. The direct mode of discovering this adulteration is by mixing the suspected salt with dilute sulphuric acid. The sulphate of quinia dissolves, but the stearine remains.—Chevalier ———. *Med. Rep.* iii. 545.

24. *Alcohol derived from the fermentation of Bread.*—Dr. Colquhoun has been led, during his investigations into the nature of the panary fermentation, into the belief, that the fermentation is alcoholic, and Mr. Graham* states, that he has obtained alcohol from bread during the operation of baking. To avoid the use of yeast, which might introduce alcohol, a small quantity of flour was kneaded, and allowed to ferment in the usual way to serve as leaven.

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* *Annals of Philosophy,* N. S. xii. p. 363.

By means of the leaven a considerable quantity of flour was fermented; and when the fermentation had arrived at the proper point, formed into a loaf. The loaf was carefully inclosed in a distillatory apparatus, and subjected for a considerable time to the baking temperature. Upon examining the condensed liquid, the taste and smell of alcohol were quite perceptible, and by repeatedly rectifying it a small quantity of alcohol was obtained, of strength sufficient to burn and to ignite gunpowder by its combustion. The experiment was frequently repeated, and in different bakings the amount of alcohol obtained of the above strength found to vary from 0.3 to 1 per cent. by weight of the flour employed. When the fermented flour was allowed to sour before baking, the amount of alcohol rapidly diminished; and in all cases the disagreeable empyreuma completely disguised the peculiar smell of the alcohol, when in its first diluted state and in vapour.

25. *Preparation of Cafein.*—The following is M. Garot's method of preparing this proximate vegetable principle. Bruised coffee was twice infused in boiling water, the liquors, when cold, mixed and precipitated by acetate of lead, which gave an abundant green precipitate. The liquid, when filtered, was yellow, the excess of lead was separated by sulphuretted hydrogen, and a colourless solution obtained, which, when its free acid had been saturated by ammonia, was carefully evaporated, and yielded crystals of *cafein.* These, when purified, became silky and colourless.—*Journal de Pharmacie.*

26. *Remarks on the Electro-motive Force of certain Substances*, by Dr. Marianini.

Carburet of Iron.—In electro-motive power it yields only to certain oxides of manganese. It is augmented by immersion into acidulated water, if the plate be not wiped when withdrawn. This increase of power is not dissipated spontaneously, but is easily lost by the action of an inferior metal, as zinc. When the latter action diminishes that belonging to the carburet, it is resumed spontaneously in a few minutes. The Passau crucibles contain much plumbago, and possess the same properties if the clay has not been too much vitrified; a crucible having an internal surface of 100 square inches, and a vessel of similar form made of thin lead, produced a very useful apparatus. A small interval was left between the leaden vessel and the crucible, and the interval filled with a strong acid solution. By heating the crucible, and cooling the interior of the leaden vessel, important effects, dependent upon the difference of temperature, could be observed.

Mercury.—This metal ranges between sulphuret of lead and silver; when impure, it becomes inferior even to brass, but distillation restores it to its place.

Iron.—Its electro-motive power is changed by oxidation, but not much. Its place is always between tin and brass.

Tin is superior to lead, but the least degree of oxidation renders it inferior. *Lead*, with zinc, gives greater deviations than tin, iron, brass, or copper. There is no arrangement formed with substances so near to each other, which produces so energetic a current, and it is in this respect opposed to that presented by zinc, with gold or platina.

Charcoal, heated until it ceases to yield flame and slowly cooled, is equal to the metals in electro-motive and conducting power. It does not retard the feeblest electric current. Its electro-motive power is variable, according to the manner in which it is cooled, and also to its exposure to air.

Oxide of Tin crystallized, produces currents, with all the metals inferior to it, as zinc, lead, tin, *&c.*; but none when in communication with bodies having a superior electro-motive force, as gold, silver, carburet of iron, grey manganese, *&c.*—*Annales de Chimie*, xxxiii. 136.

27. *List of good conductors, arranged in the order of their Electro-motive power, the most powerful being first.*—Marianini.

Charcoal after long exposure to air.
Grey radiated manganese.
Oxide of manganese.
Uncrystallized iron pyrites.
Magnetic iron pyrites.
Crystallized arsenical pyrites.
Carburet of iron.
Cubic iron pyrites.
Auriferous tellurium.
Gold.
Platina.
Copper pyrites.
Lamelliform tellurium.
Grey cobalt.
Grey copper ore.
Arsenical nickel.
Charcoal slowly cooled.
Sulphuretted protoxide of iron.
Sulphuret of lead.
Red silver ore (bright).
Arsenical silver, and arsenic slightly oxidized.
Mercury
Silver.
Tarnished antimony
Arsenic.
Sulphuret of molybdena.
Crystallized oxide of tin.
Tarnished copper.
Antimony (bright).
Charcoal extinguished in water.
Nickel.
Tarnished bismuth.
Brass much oxidized.
Bright copper.
Brass.
Crystallized protoxide of iron.
Iron.
Tarnished lead.
Manganese.
Tin.
Bright lead.
Charcoal, instantly after extinction in water.
Zinc.

Annales de Chimie, xxxiii. 139.

28. *Conducting Power of different Aqueous Solutions, that of Water being* 1. Marianini.—The solutions contained 1 part of the substance to 100 parts of distilled water.

Hydrocyanate of soda	10.96
Hydrocyanic acid	18.27
Ammonia, solution of	26.45
Soda	32.6
Phosphate of potash	44.74
Subborate of soda	45.31
Phosphate of soda	46.
Tartrate of potash and antimony	50.7
Sulphate of zinc	51.64
Chlorate of baryta	53.23
Potash	55.68
Proto-muriate of iron	56.53
Nitrate of lime	57
Acetate of potash	59.2
Nitrate of baryta	60
Proto-sulphate of iron	62.26
Bi-tartrate of potash	62.4
Sulphate of magnesia	62.64
Acetate of soda	64.9
Neutral carbonate of potash	66.7
Chlorate of potash	68.9
Carbonate of soda	69.2
Benzoic acid	70.67
Melanate of ammonia	71.15
Sulphate of soda	74.2
Benzoate of potash	76.56
Nitrate of potash	78.3
Sulphate of potash	80
Muriate of soda	84.79
Alum	85
Citric acid	85.71
Acetic acid	87
Tartrate of potash	92
Tartaric acid	98.66
Sea water	100
Muriate of lime	110
Phosphoric acid, with a little phosphorous acid	127
Ferruginous muriate of ammonia	136
Oxalate of potash	149
Muriate of ammonia	150
Acetate of copper	154
Muriatic acid	164
Oxalic acid	179
Sulphuric acid	239

Deuto-sulphate of copper	258
Proto-nitrate of mercury	278
Nitrate of silver	298
Muriate of gold	307
Nitric acid	358
Muriate of platina	418

The magnetic needle will indicate the presence of 1-2000th of muriate of soda, added to distilled water. The conducting power of alcohol is to that of distilled water as 0.323 to 1. It is augmented by the addition of salts, but less so than water.—*Annales de Chim.* xxxiii. 152.

29. *On Magnetic Phenomena, occasioned by Motion.*—An additional paragraph has been published in the last No. of the *Annales de Chimie*, to be inserted into the observations made by M. Arago, and published in a preceding No. The addition is important, as modifying the sense which would otherwise be attached to M. Arago's observations. It is as follows, and should be read between the 26th and 27th lines of page 194 of this volume:—" Although a notable diminution in the amplitude of the oscillations of a needle is constantly occasioned, as has been shown from the neighbourhood of non-metallic bodies, I am myself very much disposed to believe that the greater part of the effect is not magnetic. It is the more necessary to make, on this subject, new experiments in vacuo, because needles not magnetised have appeared to me to be subject to some influence; because the depressing action of glass, water, and paper, besides being very different to that of metals, appears to be only an action of surfaces; and because the rotatory motion of those substances has no effect, when they are separated from the magnetic needle by a screen.

30. *Perkins' High-pressure Engines.*—A report has been made by M. Girard, on a memoir by Sir William Rawson, on Perkins' high-pressure engines, read to the Academy of Sciences at Paris. The report, after enumerating the advantages stated in the memoir to be possessed by the apparatus, observes, how desirable it would be that these assertions should be supported by authentic experiments, which it appears to them they want at present, unless it be in the propulsion of balls, which comprises the *whole* of the official proofs.—*Bull. Univ.* E. vi. p. 257.

31. *Metals of the Alkalies.*—When a jet of hydrogen is directed into the residue obtained by decomposing nitre by heat, a small part of the alkali is reduced, and a large part converted into hydrate. If lamp-black, recently calcined, be thrown, in sufficient quantity, on to the same residue, to form carbonic oxide, five-sixths of the residue are volatilized, and carbonated potash remains. The effect is most perfectly obtained, by arresting the

decomposition of the salt, whilst the alkali is still combined with nitrous gas and oxide of azote.

Observing these facts, M. Van Mons has applied the residue of this decomposition to the preparation of potassium. When taken from the crucible, it is to be immediately mixed with a fifth part of its weight of recently-calcined lamp-black, introduced into a clean iron tube, and reduced in the usual way.

When the residue obtained by decomposing nitrate of soda by heat is used, a fourth part of the weight of charcoal must be added. This is a more difficult operation than that for potassium, because of the small degree of volatility of sodium.—*Bull. Univ.* A. vi. 81.

32. *Preparation of Cyanuret of Mercury.*—Mr. Turner recommends that in the preparation of cyanuret of mercury from common Prussian blue, the latter should be previously purified by digestion in diluted muriatic acid; being then washed and dried 8 parts of the purified substance with 11 of the peroxide of mercury should be boiled together in water: the former substance is entirely decomposed, and a colourless solution obtained, which on evaporation yields pure white crystals of cyanuret of mercury even to the last drop.

Mr. Turner recommends the preparation of the pure ferro-cyanate of iron from the ferro-cyanate of potash, and a per-salt of iron, as being far more economical than the Prussian blue of commerce. Prussian blue, at a shilling per ounce, contains only half its weight of pure ferro-cyanate of iron; pure ferro-cyanate of potash is only three shillings and sixpence per pound.—*Brewster's Journ.* v. 245.

33. *Spontaneous Inflammation of Iron—Protoxide of Iron.*—Professor Stromeyer states, that if hydrogen is conducted over peroxide of iron at temperatures below that at which complete reduction is effected, a partial deoxidation ensues, and the peroxide is converted into a real protoxide of iron. This oxide he asserts has never before been obtained in an insulated state. It has a dark blackish-blue colour, appearing almost black by reflected light. It stains glass blue, and is the cause of that colour in iron slag. It is combustible in a high degree. If, after its formation, it be protected from the air by an atmosphere of hydrogen till quite cold, it will take fire the instant it is placed in a saucer, so as to be exposed to the air; but, instead of passing like metallic iron into the black oxide, it is completely converted into peroxide. M. Stromeyer refers the ignition of metallic iron, observed by M. Magnus, to the presence of this protoxide, and states, that, when thoroughly reduced by hydrogen, the pure iron will not inflame at temperatures below the fusing point of

cadmium. M. Stromeyer contends for the existence of only two oxides of iron, the blue and the red.

M. Magnus, in reference to this point, still contends that it is the iron which inflames, and not a protoxide, and that oxide of iron is completely reduced by hydrogen at a temperature between the boiling point of mercury and fusing point of zinc.—*Brewster's Journ.* v. 301.

34. *Indelible Ink.*—The following receipt for indelible ink is recommended by M. Thomassin. No agent which does not destroy the fibre will act upon it. Chlorine, in various states, diminishes its colour, but exposure to light immediately restores it.

Preparation.—One ounce of dried sub-carbonate of soda, and 2 drachms of gum arabic dissolved in 4 oz. of water.—*Ink.* Two drachms of fused nitrate of silver, and 1 drachm of gum arabic, dissolved in 7 drachms of distilled water, and coloured with a little China ink.

The linen is to be moistened with the preparatory liquid, and when dry written upon with the ink by means of a pen, or printed with a block.—*Bull. Univer.* E. vi. 7.

35. *Composition of Borate of Soda.*—The borate of soda, according to the experiments of M. Soubeirons is composed of 32.416 soda, and 67.584 boracic acid. The results obtained with the borate and bi-borate of ammonia coincided with this determination.

36. *Composition of Cafeine.*—M. Pelletier states cafeine to be the substance which contains the largest quantity of nitrogen in the vegetable kingdom, and even surpassing all animal principles in this respect, except urea and uric acid. Although thus highly azotated, it is not prone to putrefaction. It consists of 46.51 carbon, 27.14 oxygen, 4.81 hydrogen, and 21.54 azote.—*Journ. de Phar.* April 1826.

37. *Salt of St. Ubes' (Setubal) in Portugal.*—St. Ubes' salt is the best known for the curing of fish. It is obtained from the sea-water of salt-marshes. It occurs in large white grains, has a slightly bitter taste, and, when heated, evolves an empyreumatic oil. Two specimens, analysed by M. Berthier, gave the following result:—

Muriate of soda	.7500	.884
Sulphate of magnesia, crystallized	.1200	.076
Sulphate of soda, ditto .	.0974	
Sulphate of lime	.0100	.010
Water and oil	.0226	.030
	1.0000	1.000

The sulphate of lime, in these specimens, is in a state of mechanical mixture, and may be washed out as white crystalline grains.

Although it has been generally supposed that the purest salt is the best, numerous facts oppose the conclusion. Cattle like salt having a bitter taste; in consequence of which circumstance, the method of crystallization, by evaporation on cords in the air, was abandoned in Savoy, although the process was economical, and yielded a pure salt. The preference given by fish-curers to the St. Ubes' salt is attributed by M. Berthier to the superiority it possesses in consequence of the presence of the sulphates; and he points out how easily similar salt could be prepared from sea-water, and the importance of experiment relative to the determination of those proportions which would ultimately be found most beneficial.—*Annales des Mines*, xiii. 225.

III. NATURAL HISTORY.

1. *Bitberg Meteoric Iron.*—This iron, according to an analysis made of it by Stromeyer, contains, iron 81.8, nickel 11.9, cobalt 1.0, manganese 0.2, sulphur 5.1, per cent.

2. *Phosphorescent Fluor-Spar.*—At a recent meeting of the Philomathic Society of Paris, a specimen of fluor-spar was exhibited by M. Becquerel, which had been found in granite, in Siberia, and sent by M. Leman. It shines in the dark, when warmed, with a remarkably strong phosphorescent light, increasing as the temperature is raised. The light augments when it is plunged in water, and in boiling water the spar becomes so luminous, that the letters of a printed book could be seen near the glass vessel containing it. On boiling mercury it emitted such a light as to enable a person to read printing at the distance of five inches. M. Eyries mentioned, at the same meeting, the statement of Sir John Mandeville, that, at the entrance of a town in Great Tartary, were two columns surmounted by stone which shone brightly in the dark.

3. *Phalaropus Lobatus, or Grey Phalarope.*

Sir, *Clungunford, Nov.* 28, 1826.

Having observed, in the 10th Number of the *Quarterly Journal*, an account of one or two scarce birds having been killed in different parts of the kingdom, I am induced to acquaint you that I have in my collection a fine specimen of that rare and curious bird, the grey phalarope, (*Phalaropus lobatus* of Latham,) killed the middle of last month near Condover, a village situate about four miles south of the town of Shrewsbury. The specimen differs in some respects from the figure and description given by Bewick, and answers more nearly, though not exactly, to one of the varieties described by Col. Montagu, in the Appendix to his *Ornithological Dictionary*, and is an additional instance of the diversity of markings to which this species is subjected. The bird was presented to me by a friend, who fortunately heard of the capture; and the account I received with it was, that it was struck down by a labouring man with a spade, as he was going to his work near the village of Condover." It is probably a solitary instance of this very elegant species having been met with so far inland. It has been beautifully preserved by Mr. Shaw of Shrewsbury, a person particularly excelling in the art; and who has taken every care, in the drying of the specimen, to develop its most striking character, (*viz.*) the lobated feet, which, on account of the great delicacy of the membranes, is not very easily attained.—I have the honour to be, &c.

JOHN ROCKE,
Rector of Clungunford, near Ludlow, Salop.

4. *Titaniferous Iron Ore of Maisdon (Loire Inférieure).*—M. Berthier finds this substance to contain oxide of iron 44 parts, oxide of titanium 9, silica 34, magnesia 10, alumine 10 *per cent.* —*Annales des Mines*, xiii. 217.

5. *Anatase, native Oxide of Titanium.*—A number of pale-yellow crystals, obtained from the diamond mines of Brazil, have been sent to M. Vauquelin. They varied in size from that of a grain of millet to that of a pea. They were transparent, and had the same colour, aspect, and particular lustre, as rough diamonds. They permitted of the action of a file. M. Brogniart, who examined their forms, suspected them to be crystals of Anatase; and a chemical examination by M. Vauquelin showed them to be perfectly pure oxide of titanium. Hence it appears that brown or blue is not the proper colour of anatase, and that this substance, which has until now only been found in primitive rocks, occurs in detached crystals in the soil which contains the diamonds of Minas Geraes in Brazil.—*Ann. des Sciences Nat.* ix. p. 223.

A New Edition (being the twelfth) of the CHEMICAL CATECHISM has just appeared, its correction and revision having been completed by the late Mr. PARKES, very shortly before his death. A brief memoir of the author has been added by his son-in-law, Mr. Hodgetts, and his portrait, an excellent likeness, is prefixed to the volume.

Mr. C. F. Partington, of the London Institution, has lately published a New Edition of his History of the Steam-Engine, including some useful additions. We have already spoken favourably of this work, which contains, in a condensed form, a vast quantity of useful practical information upon the subject, and the plates are of the first order.

Messrs. Longman and Co. have just published a New System of Astronomical Mnemonics, by Mr. R. Machan. It is an essay well calculated to initiate young persons in astronomy, by the aid of a system of artificial memory, resembling that taught by Mr. Feinagle. The details of Mr. Machan's method are, however, quite original.

A New Edition of the Meteorological Essays, by John Frederick Daniell, Esq., F.R.S., is in the press. This edition, besides the former Essays upon, 1. The Constitution of the Atmosphere; 2. The Construction and Uses of a new Hygrometer; 3. The Radiation of Heat in the Atmosphere; 4. The Horary Oscillation of the Barometer; 5. The Climate of London, with corrections and additions, will comprise Essays on the following subjects:—6. Evaporation, as connected with Atmospheric Phenomena; 7. Artificial Climate, considered with regard to Horticulture; 8. The connexion between the Oscillations of the Barometer at distant places; 9. The insinuation of Air into the Torricellian Vacuum, and the means of preventing the gradual deterioration of Barometers. It will also contain various Meteorological Observations and Remarks, and numerous Tables, Plates, and Diagrams.

Dr. Arnott's work on General and Medical Physics will be published on the 1st of February, with numerous engravings on wood. It is a system of *Natural and Experimental Philosophy*, with strictly scientific arrangement, but made easily intelligible to those who have never learned, or who have forgotten, the mathematics. In addition to a great mass of illustrations from general nature and the arts, adapted to the present more comprehensive scale of a liberal education, it comprises many very interesting particulars, furnished by examination of the animal body under health, disease, and medical treatment, and among these there are new disquisitions and suggestions.

ART. XV.—METEOROLOGICAL DIARY for the Months of September, October, and November, 1826, kept at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

FOR SEPTEMBER, 1826.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Friday	1	48	70	29.65	29.65	S	SE
Saturday	2	53	64	29.67	29.64	NE	NE
Sunday	3	56	66	29.69	29.72	NE	NE
Monday	4	53	71	29.80	29.80	NE	NE
Tuesday	5	54	66	29.80	29.75	NE	WNW
Wednesday	6	46	64	29.50	29.00	WbN	WbS
Thursday	7	50	58	29.00	29.40	NW	W
Friday	8	47	67	29.50	29.50	W	NW
Saturday	9	46	63	29.60	29.63	WNW	WbN
Sunday	10	46	61	29.77	29.90	W	W
Monday	11	42	63	30.00	30.04	W	W
Tuesday	12	45	67	30.04	30.00	W	W
Wednesday	13	47	61	29.92	29.82	W	W
Thursday	14	49	65.5	29.77	29.80	W	NW
Friday	15	33	57	30.04	30.12	NW	NE
Saturday	16	35	60	30.13	30.03	NE	SE
Sunday	17	38	66	29.89	29.79	SE	SE
Monday	18	48	60	29.79	29.70	SE	NE
Tuesday	19	55	67	29.66	29.76	E	E
Wednesday	20	57	59	29.76	29.79	E	NE
Thursday	21	50	60	29.90	29.93	NE	NE
Friday	22	38	58	29.97	29.97	EbS	EbS
Saturday	23	34	58	29.97	29.87	E	ESE
Sunday	24	34	62	29.67	29.68	ESE	E
Monday	25	54	66	29.58	29.69	SE	SW
Tuesday	26	55	66	29.58	29.65	SE	SW
Wednesday	27	54	66	29.65	29.85	W	SW
Thursday	28	52	66.5	29.93	30.00	SW	WSW
Friday	29	45	62	30.00	30.00	W	S
Saturday	30	50	67	29.60	29,63	S	W

FOR OCTOBER, 1826.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Sunday	1	43	61	29.68	29.68	W	NW
Monday	2	36	58	29.77	29.80	W	W
Tuesday	3	43	59	29.79	29.77	W	W
Wednesday	4	40	54	29.65	29.65	W	W
Thursday	5	34	54	29.67	29.68	W	NbW
Friday	6	30	50	29.90	29.99	NW	W
Saturday	7	32	59	29.97	29.82	SW	SW
Sunday	8	51	59	29.80	29.64	W	SSW
Monday	9	40	54	29.64	29.63	SW	W
Tuesday	10	41	62	29.60	29.66	S	W
Wednesday	11	55	63.5	29.82	29.90	W	WSW
Thursday	12	56	64	29.90	29.86	W	WbS
Friday	13	56	63	29.84	30.00	WSW	NW
Saturday	14	30	55	30.09	30.00	NW	SE
Sunday	15	49	62	29.81	29.61	SE	SE
Monday	16	44	61	29.49	29.57	SE	W
Tuesday	17	36	58	29.78	29.83	W	SE
Wednesday	18	36	60	29.83	29.88	SE	E
Thursday	19	51	59	29.88	29.88	E	E
Friday	20	53	59	29.86	29.84	E	E
Saturday	21	55.5	65	29.84	29.84	E	SE
Sunday	22	50	60	29.80	29.77	SE	SE
Monday	23	56	62	29.77	29.77	SE	SSW
Tuesday	24	50	60	29.77	29.63	WSW	SSW
Wednesday	25	51	57	29.30	29.28	SW	W
Thursday	26	40	50.5	29.28	29.47	W	W
Friday	27	37	55	29.50	29.68	W	WNW
Saturday	28	38	52	29.90	29.97	NW	W
Sunday	29	43	54	29.97	29.91	W	W
Monday	30	48	56	29.95	29.88	E	WSW
Tuesday	31	45	50	29.90	29.85	WNW	W

FOR NOVEMBER, 1826.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Wednesday	1	40	48	29.65	29.55	WbN	NW
Thursday	2	37	47	29.70	29.80	NW	NNW
Friday	3	39	49	29.86	29.80	N	NbE
Saturday	4	45	48	29.78	29.74	NE	NE
Sunday	5	41	48	29.78	29.77	NE	NE
Monday	6	43	44	29.69	29.73	WbN	NE
Tuesday	7	26	39	29.80	29.87	NW	NW
Wednesday	8	28	38	29.93	29.92	W	NW
Thursday	9	26	39	30.00	30.08	NW	N
Friday	10	25.5	47	29.95	29.80	W	W
Saturday	11	40	53	29.67	29.50	W	S
Sunday	12	37	45	29.49	29.38	SW	W
Monday	13	30	43	29.33	29.00	W	SE
Tuesday	14	32	40	28.97	29.08	W	SE
Wednesday	15	33	41	29.44	29.58	W	NW
Thursday	16	26	38	29.79	29.83	W	SW
Friday	17	35	45	29.70	29.88	SE	SE
Saturday	18	33	45	30.00	30.03	NE	NE
Sunday	19	40	45	30.07	30.12	NE	NE
Monday	20	39	44	30.27	30.32	NE	NE
Tuesday	21	39	43.5	30.35	30.35	NE	NE
Wednesday	22	32	44	30.30	30.20	NE	NW
Thursday	23	37	45.5	30.03	29.77	NW	WbN
Friday	24	38	41	29.40	29.22	WbN	W
Saturday	25	27	37	28.98	28.98	WbS	W
Sunday	26	21.5	34	29.20	29.20	W	W
Monday	27	24	33	29.50	29.60	W	WbS
Tuesday	28	27	42	29.60	29.46	SW	SbE
Wednesday	29	42	47	29.20	29.20	WbS	SW
Thursday	30	33	44	29.20	29.28	SSW	SW

INDEX.

Acid and salts (New), obtained during the preparation of potassium, 288

Adhesion of Glue, notice of, 183

Aërial Light and Shade, on a peculiar perspective appearance of, 81

—— phenomena on the top of the Pic du Midi, 223

Air-pump without artificial valves, notice of, 181; new mode of working, 182

Alcohol, derived from the fermentation of bread, 401

Alkalies, metals of, 405

Analysis of scientific Books, 95, 142; Transactions of the Royal Society, 329; Horticultural Society, 338; Mr. Peyron's account of Egyptian Papyrus, 345

Anatase, native oxide of titanium, 409

Animal matter, instrument for ascertaining its presence in the atmosphere, 219

Apartments in low and damp situations, improvement of, 185

Architecture (Naval), papers on, and other subjects connected with naval science, analysis of, 95

Astronomical and nautical collections, 143, 350

Balance (Hydrostatic), cheap and delicate one, notice of, 181

Balard, on a new simple body, brome, 384

Baryta (Sulphate of), and water, peculiar states of, 213

Berthier (M.) on the preparation of pure nickel, 395

Bitumens (elastic), analysis of, 228

Blood, morbid alterations of the, 228

Bone, strength of, 371

Boracic Acid (fused), light evolved by the rupture of, 374; borate of soda, composition of, 407

Brande (W. T., Esq.) outlines of geology by, 51, 249

Brewster (Dr.) on peculiar states of sulphate of baryta and of water, 213

Brome, a new simple substance, account of, 384
Bull (Mr.), his method of making charcoal, 184

Cafeine, preparation of, 402; composition of, 407
Capillary Thermometer (delicate), notice of, 192
Carbon, on a new form of, supposed to be its pure metallic basis, 204
Carbonic Oxide (pure), preparation of, 392
Caventon (M.) on starch, and its commercial varieties, as sago, tapioca, &c., 216
Celestial body (new), notice of, 372
Cerium, sulphuret of, 398
Chalk and Pencil Drawings, mode of fixing, 220
Chances, practical application of the doctrine of as it regards the subdivision of risks, 84
Charcoal (Animal), purification of pyroligneous acid by, 218
———, improved method of making, 184
Chlorine and Fluorine, new compounds of with chromium, 211
Chromium, new compound of, with fluorine and chlorine, 211
Coal, observations on the geological history and formation of, 293
Colquhoun, (Dr.) on supposed metallic basis of carbon, 204
Comet of short period, notice of, 372
Combustion, spontaneous, of lamp-black, 391
Compasses, remarks on local influence of, 314
Crater of Kirauca, on the island of Hawaii, description of, 221
Creuze (Messrs. W. M. and A.) papers by, on naval architecture, analysis of, 95
Crystals, relation of the forms of, to their dilatation by heat, 198
——— purification of, 399

D'Arcet and Thenard, (MM.) on fatty bodies, as an hydrofuge in painting, 184
Depression of Mercury in Thermometers, notice of, 193
Doctrine of Chances, practical application of, 84
Drawings in Chalk and Pencil, method of fixing, 220
Dumas (M.) on new compounds, fluorides, chlorides, &c., 211
Dyed Stuffs, on the method of proving, 219

Elastic Bitumens, analysis of, 228
Electro-magnetism, new facts in, 383
Electro-motive force of certain bodies, 402
Engine, (explosive,) new, notice of, 182
Engines, high-pressure, 405
Equations, on improvements in the solution of, by continued fractions, 67, 285
Eve, (Mr. J.), account of a new hydraulic machine invented by, 247
Explosive Engine, new, notice of, 182

Faraday, (M.), on a peculiar perspective appearance of aërial light and shade, 81; on the confinement of dry gases over mercury, 220
Fatty Bodies, employment of, as a hydrofuge in painting, 184 ; on the distillation of, 218
Filtration, alteration of oil by, 183
Fluorine and Chlorine, new compounds of chromium with, 211
Fluor spar, phosphorescent, 408
Forms, (primitive), variation of, 199
Freycinet's, (M. de), results of his experiments on the length of the pendulum, 143
Fulminate of Silver, decomposition of, 393

Gases, compression of, law of the rate of, 197
—— electricity of, and causes of atmospheric electricity, 198
—— on the solution of, in water, 204
—— (dry), on the confinement of, over mercury, 220
Genus Hirudo, observations on Professor Carena's monograph of, 38
Geology, outlines of, 51, 249
Glue, adhesion of, 183
Gold, (Mosaic), notice of, 397
Goring, (Dr.), on Mr. Tulley's thick aplanatic object-glasses, 265
Graham (Mr.), on the solution of gases in water, 204
Granville (Dr.), his septometer, notice of, 219
Gunpowder, ignition of, by the electric discharge, 200—380

Hair-Hygrometer, remarks on, 28
Harvey (Mr.), anomalous case of vision in regard to colours, by, 188
Horner (W. G., Esq.), on improvements in the solution of equations by continued fractions, 67—285
Horticultural Society, proceedings of, 323
Hydraulic Machine (new), account of, 247
Hydrofuge, in painting, employment of fatty bodies as, 184
Hygrometer (Hair), remarks on, 28
Hypo-nitrous and Sulphuric Acids, new compounds of, 392

Incandescence of Platina, substances by whose vapour it is continued, 202
Ink, sympathetic, notice of, 401
—– indelible, 407
Iodine, presence of in mineral-waters, 206
Iron, spontaneous inflammation of, 406
—— meteoric of Bitberg, 438 ; titaniferous, 409

Johnson (Dr.), his observations on Professor Carena's monograph of the genus hirudo, 38

Lamp-Black, spontaneous combustion of, 391
Light evolved by fused boracic acid, 374
Lime, mode of burning without kilns, 183
Local Influences on Compasses, remarks on, 314
Lunar Tables, notice respecting, 145

Mac Culloch (Dr.), general view of the origin, character, and disposition of unstratified rocks and veins, 1
———— on the geological history and formation of coal, 293
Machine, new hydraulic, account of, 247
——— electrical, deviation of the magnetic-needle by, 380
Mackintosh (Mr.) on the power of the sun's rays, 224
Magnus (M. G.) on the property possessed by certain metallic powders of inflaming in the air, 201
Manganese, native black oxide of, with some observations on the peroxide of lead, and on chlorine, 231

Mémorial de l'Artillerie, &c., No. I., analysis of, 113

Magnetic Phenomena occasioned by motion, notice of, 193—405. Influence of the sun, 196. Needle, deviation of by the electricity of machines, 380

Mariotte's law of the rate of compression of gases, 197

Medico-Botanical Society, proceedings of, 327

Mercury, confinement of dry gases over, 220

——— cyanuret of, preparation of, 406

Metal, on the precipitation of, from solution, by other metals, 202. Conducting power of, for electricity, 374. Metallic wire, intensity of the electro-dynamic force in, 379. Fusible metal, and refrigerating metallic combination, 397. Metal of the alkalies, 405

Metallic (powders), property possessed by certain, of inflaming in the air, 201. Supposed pure base of carbon, 204

Mineral Waters, presence of iodine in, 206 ; formation of pyrites in, 398.

Monochromatic light, 374.

Monograph on the genus Hirudo, observations on Professor Carena's, 38

Morey, (Mr.) his new explosion-engine, 182

Mortars, note relative to the last memoir on, 400

Mosaic gold, notice of, 397

Motion, magnetic phenomena occasioned by, 193

Mullen, (Mr. J. Mac) observations by, on black oxide of magnetic peroxide of lead and chlorine, 231.

Naval architecture and science, analysis of papers on, 95

New compounds, fluorides, chlorides, &c., 211

Nickel, (pure) preparation of, 395

Object-glasses, (aplanatic) account of, 265

Oil, alteration of by filtration, 183

Papyrus, Egyptian, account of, 345

Paving, the present mode of, with a plan for its improvement, 261

Pendulum, results of experiments on the length of the, 143-152
Perspective, appearance of aërial light and shade, 81
Pic du Midi, aërial phenomena on the top of the, 223
Phalaropus Lobatus, notice of, 408
Plaster statues and bas-reliefs, preparation of, 186
Platina-wire, substances that continue the incandescence of a coil if held over their vapour, 202
Platina, on the native locality of, 225
Potassium, advantageous preparation of, 206 ; new acid and salts obtained during, 208
Pouillet, (M.) on the electricity of gases, and the causes of atmospheric electricity, 198
Prevost, (Professor) on the magnetic influence of the sun, 196
Prinsep, (Mr.) on the hair-hygrometer, 28
Proving the colours of dyed stuffs, notice of, 219
Puzzolanas and hydraulic mortars, on the mode of action of, 214
Pyroligneous acid, purification of by animal charcoal, 218
Pyrites, formation of in a mineral water, 398

Rattle-snakes, notices of, 227
Ritchie, (Mr.) his air-pump and hydrostatic balance, 181 ; new mode of working an air-pump by continued motion, 182
Rocks and Veins, unstratified, general view of the origin, characters, and disposition of, 1
Rocks, decomposition of, 51
Royal Society, proceedings of, 316

Salt of St. Ube's (Setubal), Portugal, 407
Salts, variation of the primitive forms of, 199
Septometer, notice of, 219
Silver, fulminate of, decomposition of by sulphuretted hydrogen, 393
Solid particles, mutual re-action and aggregation of, 199
Sound, transmission of, 372
Starch, *Sago*, and *Tapioca*, observations on, 216.

Sulphate of magnesia and *bi-carbonate of soda*, re-action of, notice of, 400; sulphate of quinia, adulteration of, 481
Sulphate of Soda (anhydrous), 399
Sun's Rays, power of, 224
Svanberg (Jöns), experiments on the length of the seconds' pendulum and the specific gravity of water, 152
Syphon hydrometer, notice of, 371

Taylor (Rev. R.), analysis of his view of nature, &c., 134
Tenacity of vegetable life, 229
Theatres, new method of lighting, 371
Thermometer (delicate), capillary, 192; gradual depression of mercury in thermometers, 193
Thomson (Mr. D.) on human tables, 145
Titanium, native oxide of, 409
Transparency of the ocean, 224
Tulley (Mr.), account of his aplanatic object-glasses, 265
Turner (Mr.), on the presence of iodine in mineral waters, 206

Vegetable life, tenacity of, 229
"*View of Nature*," analysis of, 134
Vicat (M.), on the mode of action of puzzolana and hydraulic mortars, 214
Vision, anomalous case of in regard to colours, 188

Water, specific gravity of, and experiments for determining the length of the seconds' pendulum, 152
Wohler (M.), advantageous preparation of potassium by, 206

LONDON:
Printed by WILLIAM CLOWES
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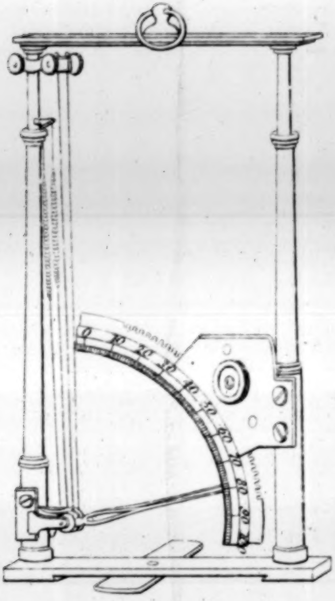

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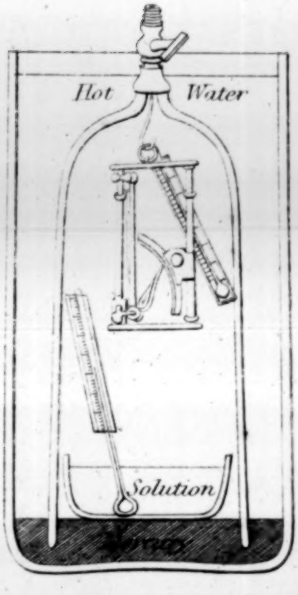

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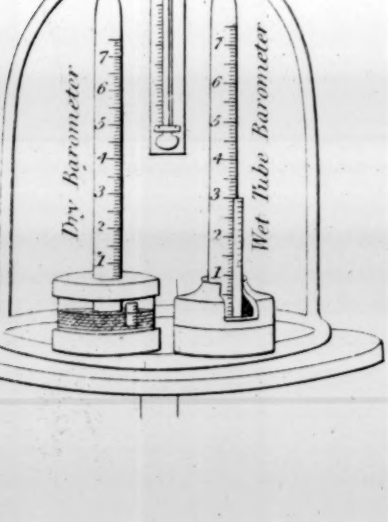

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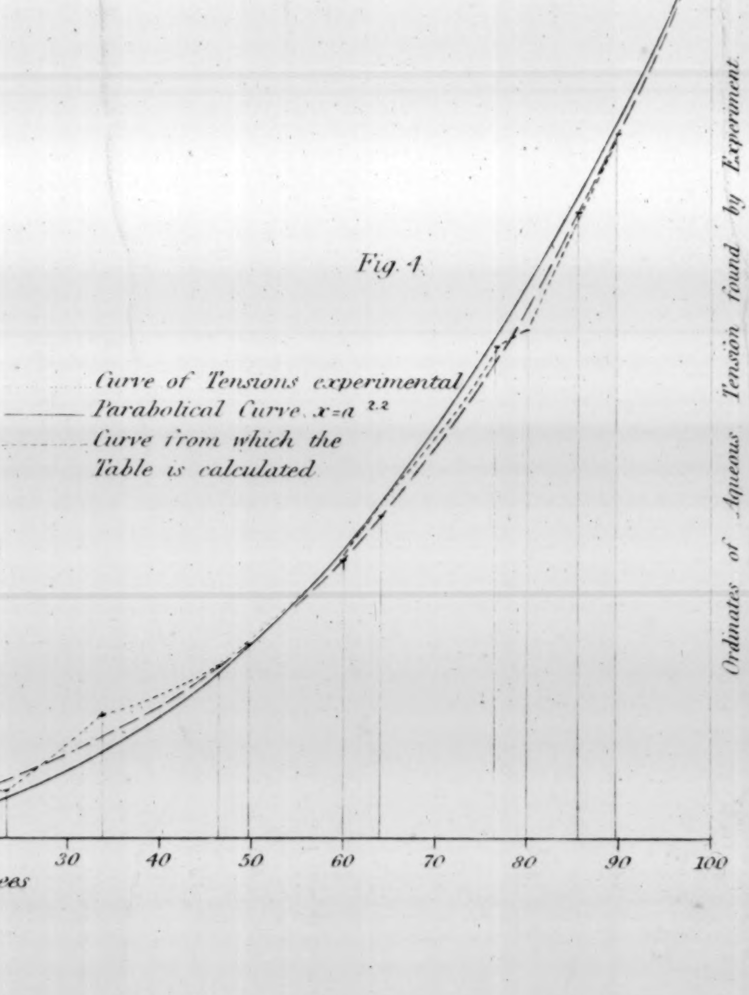

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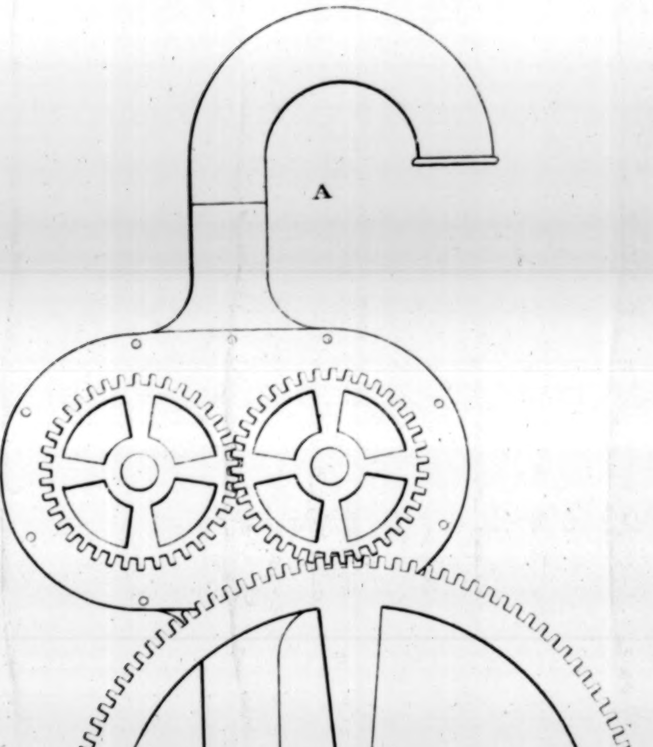

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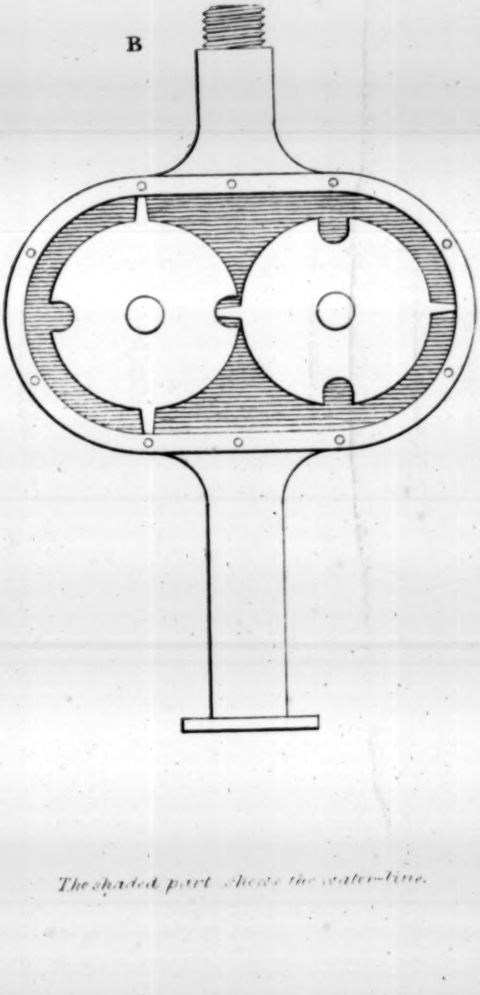

The shaded part shews the water-line.

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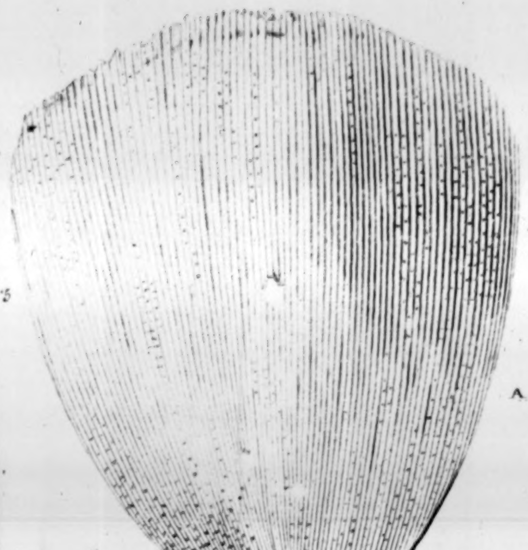

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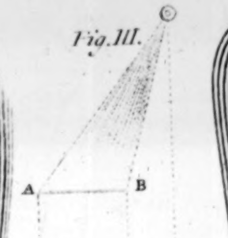

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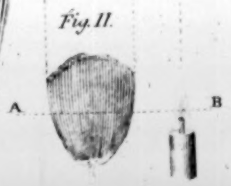

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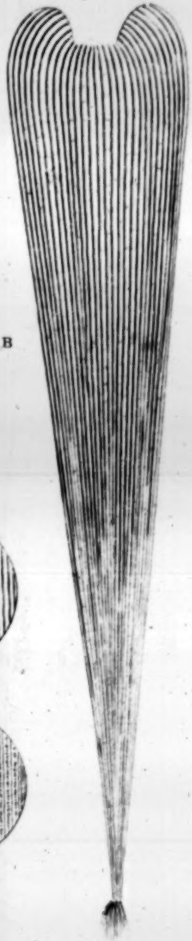

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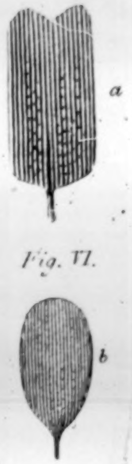

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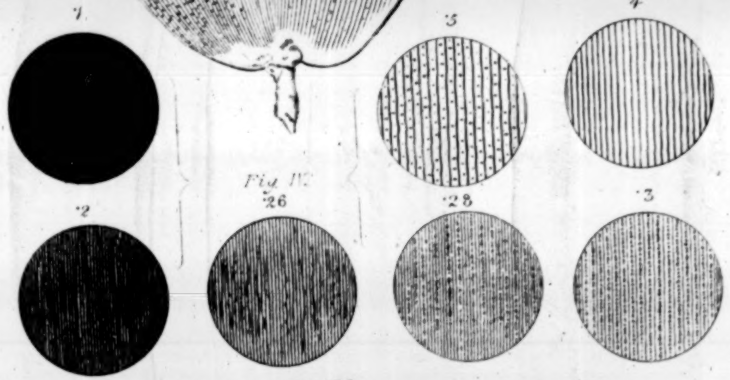

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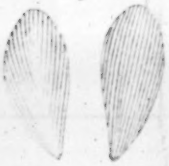

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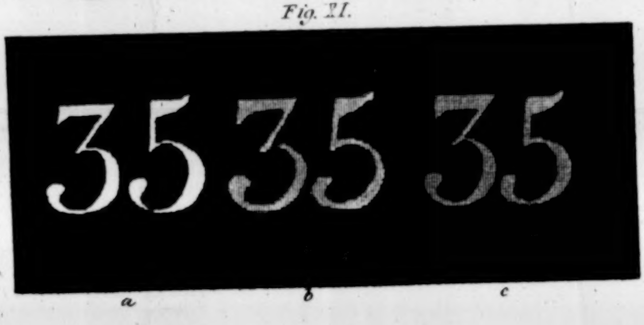

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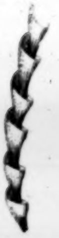

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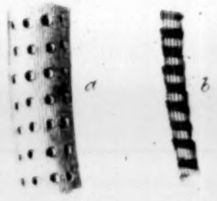

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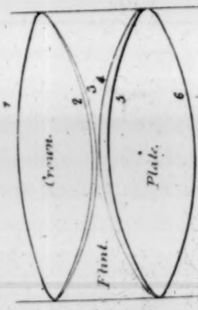

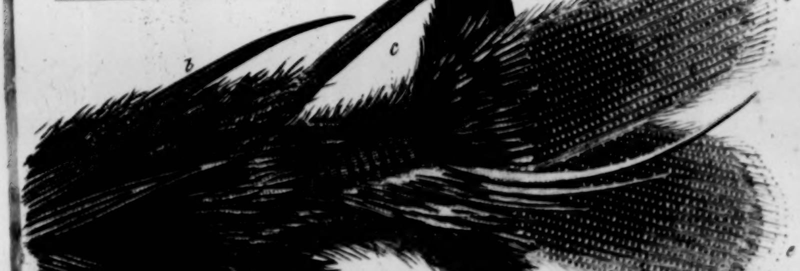

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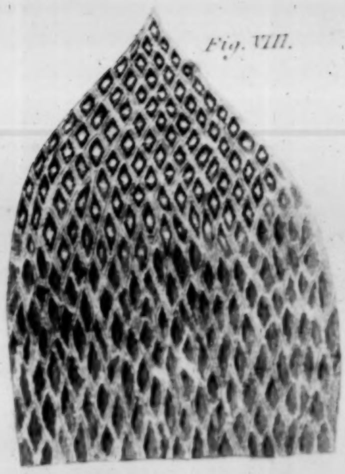

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